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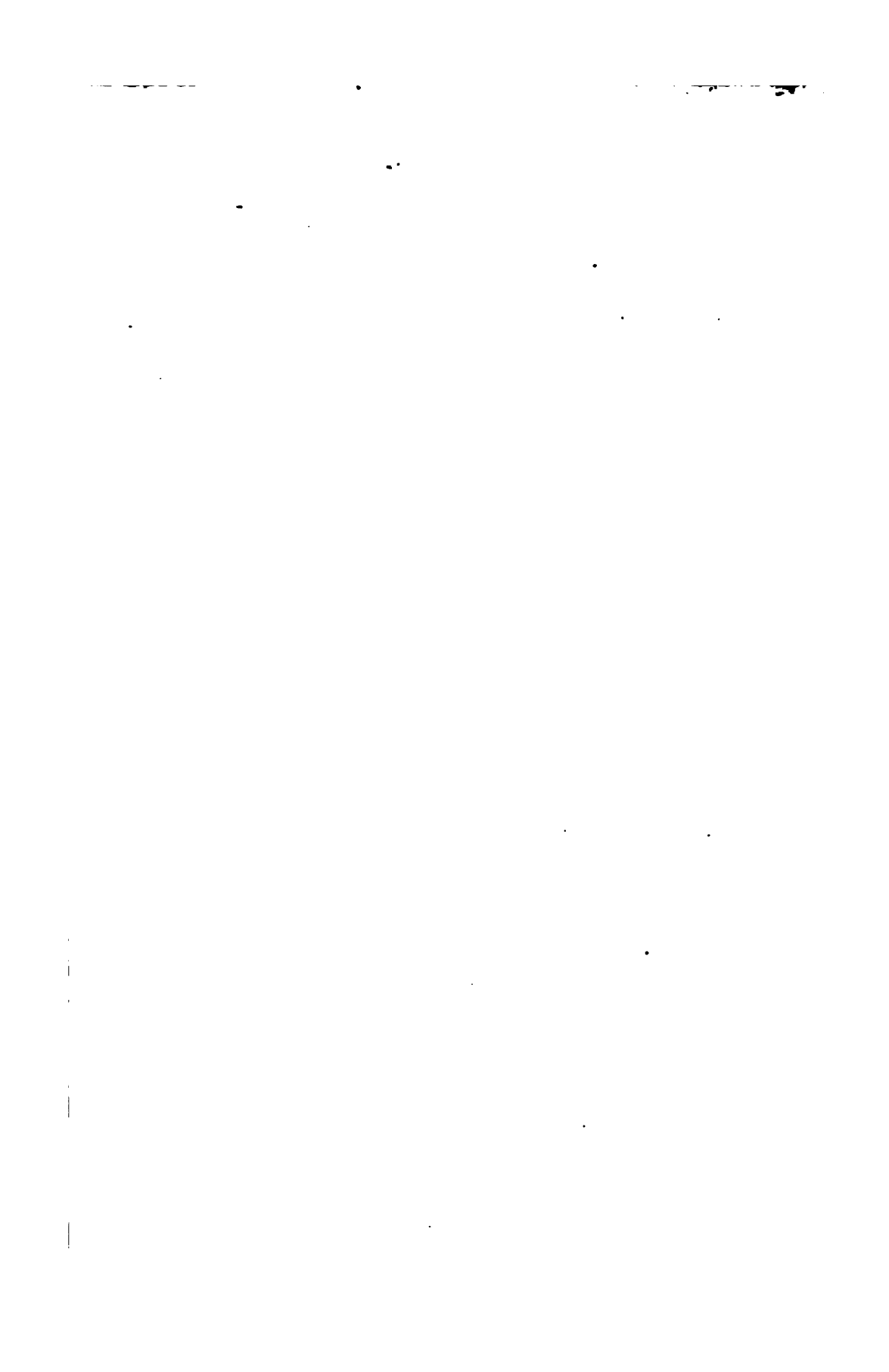
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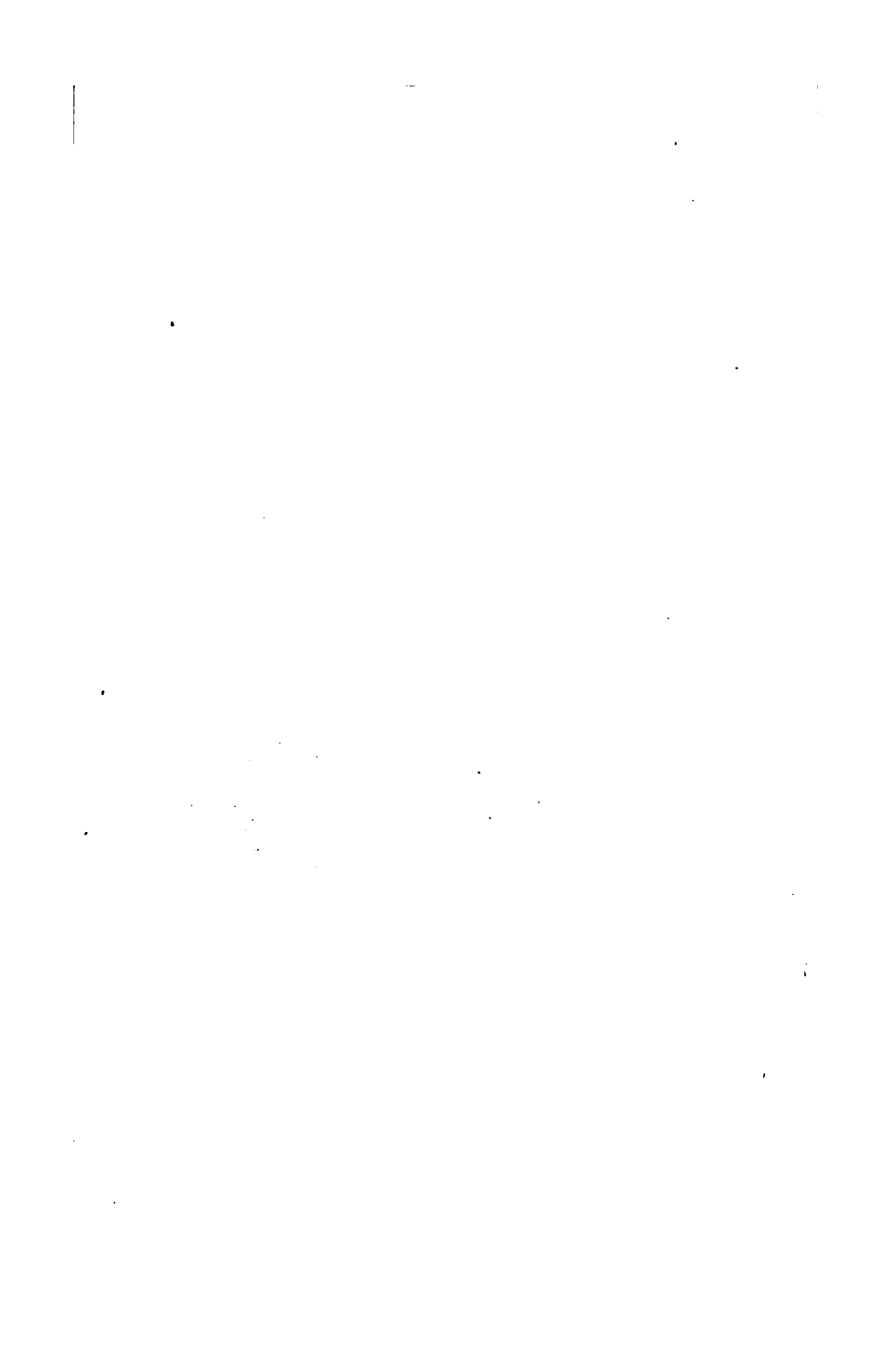


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No. 25. NEW SERIES.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Contributions to Pathology and Rational Medicine.* By
JOHN HUGHES BENNETT, M.D., F.R.S.E., &c.

No. XV.—CLINICAL AND HISTOLOGICAL RESEARCHES ON CAN-
CEROUS AND CANCROID GROWTHS.—(Continued).

Colloid Cancer.

THE term colloid (from *colla*, glue) is applied to collections of gelatine which not unfrequently occur in the body. They resemble glue, calves' foot jelly, gum arabic, and similar glutinous compounds, of greater or less consistence, and are found in masses varying in size from a minute point to the size of the human fist, or even larger. In colour colloid matter may be greyish, of a light or dark yellow, brownish, reddish, and rarely green or black. It may be clear and transparent like amber, or semi-transparent and opaque, resembling honey. It may be found disseminated in a fibrous texture, giving it a pearly aspect, or it may constitute the contents of distinct cysts. It is one of the most common constituents of compound encysted growths of the ovary, and is not unfrequently seen in cysts of the kidney and follicular swellings of the skin.

Colloid matter, distributed throughout a fibrous structure, presents numerous loculi, filled with a grey, or amber-coloured, glutinous matter, sometimes transparent, at others opalescent or semi-opaque. On examination with a microscope, the matter is occasionally found quite structureless, or exhibits only a finely molecular appearance. Under these circumstances the term *colloid tissue* has been applied to it. At other times numerous nucleated cells, in various stages of development, are found in it as a blastema, and we observe that the

NEW SERIES.—NO. XXV. JULY 1848.

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growth has a tendency to spread. This is *colloid cancer*—the *gum* or *gelatiniform cancer* of some authors—*carcinoma alveolare* (Muller)—*cancer areolaire pultacé* (Cruvelhier). It occurs most commonly in the peritoneum surrounding the stomach and large intestine, in the omentum and ovaries. It is occasionally found in the bones, and rarely in the uterus, liver, and kidneys. The following Observations illustrate the nature of this form of cancer:—

OBSERVATION XXIV.—*Colloid Cancer of Omentum, and the Peritoneum lining the various Abdominal Viscera—Compound Encysted Colloid Tumours of Ovaries.*

Mrs K., *et.* twenty-eight, had no children, consulted Mr Woodhead in October 1845, considering herself to be in a pregnant state. She thought that she had been in the family way about seven months; during which time, however, the catamenia had appeared regularly, but were scanty. There was a visible enlargement of the abdomen, but no particular examination was made. In November she complained of pains in her back and abdomen, and passed very restless nights. These symptoms were treated by palliatives, and attributed to the spurious pains so common in advanced pregnancy. In the beginning of December an examination of the abdomen was made, which was considerably enlarged, so much so as to resemble a woman about the eighth or ninth month of utero-gestation. A distinct fluctuation was perceived, and an indurated tumour which remarkably resembled a fetal head. About the commencement of January 1846 she was examined *per vaginam*, when the *os uteri* was felt unusually high, which led to the suspicion that she was not pregnant. At this time the peritoneal fluid was much increased in amount, and the abdomen was very tense. The respiration was considerably embarrassed, and the nights rendered restless from dyspnoea. About the middle of January Dr Simpson was consulted. The following day paracentesis was performed at the umbilicus, which was unusually prominent, and a wash-hand basinful and a half of fluid was drawn off. This fluid was of a reddish colour, and contained numerous flocculi and white nodules. The former were seen on a microscopic examination to be composed of several corpuscles, both nucleated and granular, similar to those hereafter to be described, entangled together in groups by numerous filaments. There were also many blood globules. The white nodules were entirely composed of filamentous tissue, containing several fusiform corpuscles.

The breathing was much relieved by the tapping, but from this time she could not carry on her usual occupation, which she had hitherto done. The pains in the back and abdomen continued: a febrile action was excited; the pulse rose to above 100; the nights continued restless: no vomiting; and a cachectic state made its appearance. The wound did not close, but continued to discharge a reddish fluid, which afterwards became dark, often mixed with flocculi, and latterly with a quantity of jelly-like substance. About the second week in February the tumour was felt hard, unequal on the surface, and apparently attached to the parietal walls of the abdomen anteriorly. On placing the ear over the abdomen a distinct to and fro friction murmur could be heard. Fluctuation could also be felt at the lower part of the abdomen. Two other openings now took place in the umbilicus, from which a continual discharge escaped. The other symptoms continued; the pains could only be relieved by morphia, and she sunk exhausted on the morning of March 8.

Section cadaveris, March 9, eight P.M.

The body was much emaciated; head not examined.

The thoracic organs were healthy.

Abdomen.—On cutting through the peritoneum, about a pint of dirty yellow purulent fluid escaped. The pelvis and abdomen inferiorly were occupied by an encysted tumour, which had burst anteriorly, exposing several cysts containing an amber-coloured jelly-like matter. The peritoneum superiorly and

interiorly was united to a firm tumour underneath by gelatinous adhesions, which were easily broken through. This tumour occupied the whole breadth of the abdomen, and extended from the ensiform cartilage to midway between the umbilicus and pubis. It varied in thickness from one to two inches, and was evidently a growth in the substance of the omentum, being attached superiorly to the stomach and transverse colon, lying over the intestines, and terminating inferiorly in a rounded edge. When removed from its attachments the tumour weighed three pounds thirteen ounces. To its anterior and posterior surface were attached numerous shreds and patches of chronic lymph. On cutting into it in various directions, it presented numerous cells, varying in size from a pin's head to that of a large pea, filled with a clear, glistening, gelatinous matter, surrounded by a firm fibrous substance or mesh-work. In some places it was of a greenish hue, in others of an ochrey or orange yellow. Here and there the meshes of the tissue were tinged of a greyish hue by black pigmentary deposits.

The peritoneal membrane lining the abdominal parietes was coated with a layer of the same colloid cancer about two lines in thickness, covered with patches and flocculi of lymph. This layer could in many places be stripped from the serous membrane below, which appeared to be healthy. Between the liver and diaphragm a mass existed half an inch thick. The spleen was surrounded by a similar layer. The lymphatic glands in the neighbourhood of the pancreas and stomach were much enlarged, and apparently quite converted into the colloid cancer. A mass surrounded the uterus more than an inch thick, circumscribing the cavity of the pelvis. The lumbar glands and *appendices epiploicæ* of the large intestines were greatly enlarged and similarly affected. On cutting into the liver, spleen, and kidneys, they were found healthy; the peritoneal membrane covering them being alone affected. The peritoneum surrounding the pyloric extremity of the stomach, and extending a third over the surface of that viscus, was upwards of an inch thick from deposit of colloid cancer. Its muscular and mucous coats were healthy. The small intestines were united together by bands of chronic lymph, of a bluish-black colour from pigmentary deposit. Anteriorly they were covered *en masse* by a layer of lymph, of greenish colour, about a line in thickness.

Both ovaries were the seat of a compound cystic tumour, that on the left side was in circumference about the size of the crown of a hat. When viewed anteriorly, it presented the appearance of a mass of trembling calves'-foot jelly, with numerous blood-vessels ramifying upon it. Shreds of the external sac were seen attached to it here and there, which had evidently sloughed, and in some places been adherent to the abdominal parietes. Posteriorly the fibrous sac was still entire, and presented numerous rounded elevations, marking out the forms of distinct cysts. On incising this mass it was found to consist of numerous cysts, the walls of which were very thin, but richly supplied with blood-vessels. They for the most part contained a trembling transparent, gelatinous substance, generally of an amber colour, but here and there of a brownish-red. Imbedded in some of this jelly, there existed opaque white masses, resembling blanc-mange, or thick cream. In other places the gelatinous matter might be observed semi-transparent, and of a light yellow colour.

The tumour of the right ovary was about the size of a small orange. In the fibrous capsule anteriorly was a round perforation the size of a fourpenny piece, from which a similar amber-coloured jelly to that which existed on the opposite side could be squeezed. On section it was found entirely full of cysts, all filled with the same gelatinous matter.

Microscopic Examination.—On making a thin section of the peculiar growth on the peritoneum, it every where presented a network of transparent filaments, forming loculi of various sizes; the filaments surrounding one opening often running in those forming others—Fig. 64. Some of these loculi contained nothing but a perfectly transparent fluid; others contained cells, generally of oval form, although a few were round or caudate. They varied in their

longest diameter from the 1-50th to the 1-30th of a millimetre in length. They all contained a nucleus of oval shape, varying in its longest diameter from the 1-200th to the 1-80th of a millimetre in length. The larger of these contained an oval nucleolus. These cells occurred in groups, and many of them floated loose in the fluid squeezed from the section—Fig. 64. Here and there numerous fat granules, and compound granular masses and corpuscles, existed in groups. On the addition of acetic acid the fibres became somewhat more transparent, the external walls of the cells were partly dissolved, while the nucleus was unaffected. The dirty yellow purulent matter was composed of broken down pus corpuscles, mixed with innumerable granules and molecules.

Fig. 64.

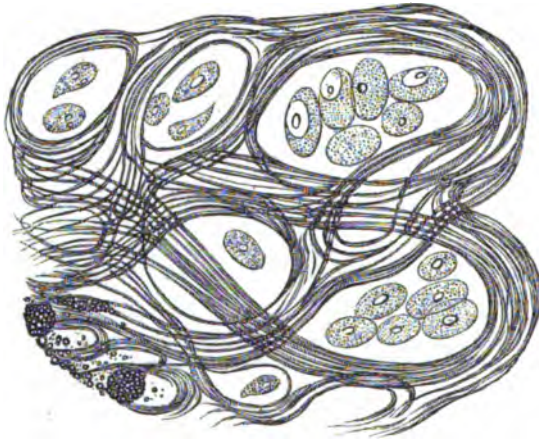


Fig. 64. Appearance of a thin section of the colloid cancer of the peritoneum, described Obs. XXIV.

The lymph attached to the omental tumour and parietal walls of the abdomen was composed of filamentous tissue, interspersed with the same broken down corpuscles and granules which have just been alluded to. It was also studded over with crystals of the triple phosphate.

The amber-coloured transparent jelly within the cysts of the ovaries, at first appeared to be structureless. On careful examination, however, with the rays of light directed obliquely, very faint cells could be distinguished of an oval form, varying in their longest diameter from the 1-80th to the 1-25th of a millimetre in diameter. Some of these were distinctly nucleated—Fig. 76. A few oily granules could also be seen isolated and in groups.

The white opaque creamy matter in the cysts was entirely composed of minute granules and molecules, united together by delicate filaments. These granules were of two distinct sizes. Those in one group being generally about the 1-500th, those in the other being about the 1-600th or 1-700th of a millimetre in diameter—Fig. 74. These granules were densely grouped together, and in some places constituted granular masses, similar to those seen in inflammatory exudation.

In the light yellow semi-transparent gelatinous matter delicate filaments could be seen crossing each other, together with numerous faint cells, round in shape, about the size of pus corpuscles, containing from one to six granules—Fig. 75.

The walls of the cysts themselves were composed of fibrous tissue, more or

less covered with numerous granules. In a few places only could granular cells be seen.¹

Remarks.—This case presents the largest quantity of colloid cancer present in one individual, that, so far as I am aware, has ever been recorded. It is worthy of observation, that it was only found on the free surface of the peritoneum surrounding each organ, but leaving these untouched. Even the masses which covered the pyloric end of the stomach in no way produced constriction of the organ, or disease of its muscular coat. I am inclined to think, that the colloid masses in the ovary were in nature the same as those found in the fibrous growth of the peritoneum. Although not so much loaded with cells, it contained several in progress of development. A series of preparations illustrative of the morbid changes in this case, is preserved in my collection. The colloid matter of the ovary, which when recent was perfectly transparent, has been rendered opaque and white by the action of the spirit, while that on the peritoneum has remained unaltered.

OBSERVATION XXV. — Colloid Cancer of Peritoneum, Stomach, and Ovaries.—Marion Greenhill, æt. thirty-one, was admitted into the Royal Infirmary, Oct. 17, 1846, under the care of Dr Paterson. She dated the decline of her health from a difficult labour she had about three or four months previously. About a month subsequently she began to lose flesh, and became languid and jaundiced. At this time also, a small round swelling appeared in the left iliac region. She soon after felt a swelling at the epigastric region, accompanied by a burning heat shooting through the back. The abdomen also commenced to swell. For the last six weeks the urine has been scanty, with frequent desire to micturate, and for the last two weeks she has experienced excessive vomiting. On admission, no tumours could be felt, but there was considerable distension, with some tenderness of the abdomen. There was thirst, white tongue, weak pulse, scanty urine, and constipation. The treatment was palliative only. She died at the end of November, the emaciation and debility increasing, occasional vomiting, and latterly symptoms of pleurisy and pneumonia.

Sec tio Cadaveris, Dec. 2.—Skin of yellow cachectic hue—extreme emaciation—abdomen distended by fluid.

Head was not examined.

Thorax.—Heart small—felt very dense—coronary veins very turgid. About four oz. of dark brown turbid serum, containing flocculi of lymph, were effused into the right pleura. The pleura, over the lower lobe of the same lung, was coated with patches of recent lymph. The inferior portion of this lobe was partly grey, partly red on section, from hepatization, and presented two indurated portions the size of a walnut, and of a plum colour, as if from recent extravasation of blood. There were about six oz. of serum in the left pleura; the inferior margin of the lower lobe of left lung also hepatized, but to a less extent than the right. A few patches of recent lymph also covered its pleural surface.

Abdomen.—The peritoneal cavity contained about a gallon and half of dark yellowish brown clear fluid. Under the right lobe of liver a pouch was formed by a band of membranous lymph, stretching from its fissure to the anterior abdominal wall, which contained about a pint of fluid similar in its character.

¹ For figures illustrative of the structure of encysted tumours of the ovary, the writer must refer to a paper of his inserted in the *Edin. Med. and Surg. Journal* for January 1846. A few of these are given at the end of this communication.

The whole of the peritoneal surface was of a dark red or purple colour, mottled with round and square patches of a white colour, varying in size from a small pea and under, to three-fourths of an inch square. The dark colour of the peritoneum was owing to a covering of chronic lymph, deeply tinged with blood, which could be removed in layers from the surface of the intestines. The white patches consisted of the healthy serous membrane shining through the red exudation on its surface. Scattered over the peritoneum were numerous rounded semi-transparent masses of colloid cancer, resembling in appearance gum arabic, and varying in size from a small pea to that of a hazel nut. Over the transverse or descending colon, these masses were very common, and closely aggregated together. The peritoneal surface of the diaphragm was covered with similar masses, some of them as large as an almond, and its posterior portion adhered strongly to the liver, through the medium of the same deposit, a continuation of which accompanied the vena cava in its passage through the liver, and surrounded it to the thickness of a quarter of an inch. The spleen was united to the diaphragm and descending colon by firm bands of chronic lymph, and its surface was studded over with round semi-transparent gelatinous masses, the size of a pea. Internally the above organs were all healthy, the disease being confined to their peritoneal coverings. The pancreas and kidneys were also healthy.

The pyloric end of the stomach was felt to be hard and greatly enlarged, constituting a dense tumour, and the transverse arch of the colon was closely united to it, and drawn up towards the greater curvature by a mass of colloid cancer, extending between the parts. The coats of the stomach along the lesser curvature, and the pyloric extremity, were contracted and thickened half an inch by the deposition of the same cancerous matter.

Both ovaries were converted into solid tumours, about the size of a small orange, but deeply nodulated and irregular on their surfaces. On section, they seemed composed of a sarcomatous mass. In the left, this enclosed three cavities about the size of a bean, filled with a gelatinous matter, resembling in colour and consistence trembling calves'-feet jelly.

Fig. 65.

Fig. 66.

Fig. 68.

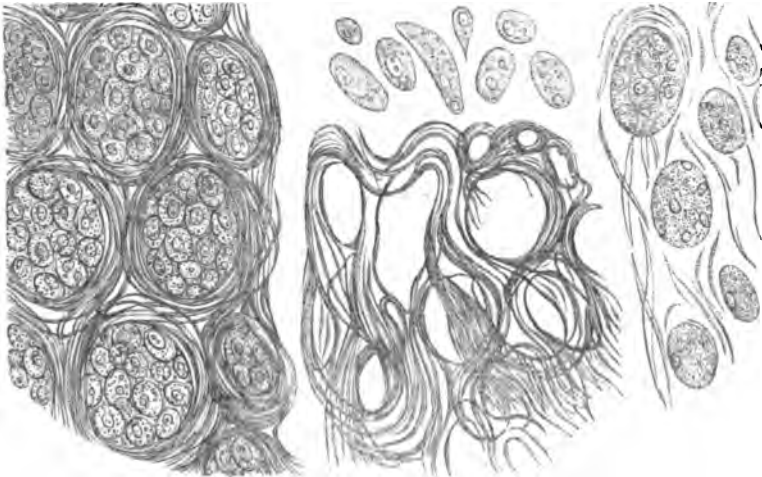


Fig. 67.

Fig. 65. Section of the colloid cancer described Obs. XXV., showing the loculi in the fibrous structure and the contained cells. Fig. 66. Several cells isolated. Fig. 67. Fibrous stroma deprived of the cells by pressure and washing. Fig. 68. Section of the growth treated with acetic acid.

Microscopic Examination.—On making a thin section of the morbid growth, it presented the same general appearance of a fibrous stroma, forming loculi, and containing cells, as in Observation XXIV. In some places, however, the loculi were completely crowded with cells, as seen in Fig. 65. The cells also presented exactly the same character as in the last case, but in some places were more numerous. Several are represented isolated, Fig. 66. Granules and compound granular corpuscles were collected together here and there in groups in the substance of some sections. Firm pressure between glasses, and washing with water, got rid of the cells from a thin section altogether, so that the fibrous stroma appeared as represented Fig. 67. Here and there also, the morbid growth seemed to be wholly composed of colloid tissue, no cells being visible in the amber-coloured fluid. On adding acetic acid, the fibrous texture became more transparent, the cell-walls were partially dissolved, while their nuclei remained unchanged.—(Fig. 68.) In the ovaries the fibrous stroma was more dense, but presented here and there a cystic structure, as in the last case. The colloid matter also was rendered turbid by acetic acid.

Remarks.—In this Observation we observe the same disease as in the last, which, however, had not proceeded to the same extent as regards quantity, although the development of the corpuscles in some places was more exuberant. On comparing Fig. 65 with Fig. 1,¹ the resemblance of structure becomes striking. On comparing the two sections when treated with acetic acid, however—Fig. 68 and Fig. 2—a marked difference is exhibited in the fibrous element of the growth. In the former, the one now under consideration, no oval or elongated nuclei were present; in the latter, exhibiting ordinary cancer of the breast, the presence of these nuclei renders it probable that the fibrous element is developed in a different manner. As far as my observations have carried me, permanent nuclei are never present in colloid tissue.

For an opportunity of examining the excised breast in the following Observation, I am indebted to Professor Miller. An account of the case was kindly given to me by Dr Cornwall of Leith.

OBSERVATION XXVI.—Colloid Cancer of the Mamma—Excision—Cure.

Miss S——, set. fifty-one, of firm stout make, robust, and of a ruddy healthy complexion, in March 1847 discovered, on trying on a pair of new stays, a tumour in her right breast. It becoming uneasy and painful, she came to Edinburgh and had medical advice; a prescription was given her for medicine to take inwardly, and for a lotion to be applied to the part. She was advised to return home to the country, and persevere in the use of these medicaments for several months.

The growth continued to increase in size. At the beginning of the present month it seemed to be about the size and shape of a walnut; its position was about an inch to the outer and upper side of the nipple, unconnected, *i. e.* not adherent, to the muscle beneath, or the skin over it; no trace of affection of the lymphatic system. In consistence it was firm to the feel, but not particularly hard; pressure caused pain; the surface was felt to be botryoidal or nodulated, as if peas were imbedded into it; poised in the hand it had not much weight; the breasts full and fat. Two kinds of pain were complained of, described as dull gnawing pains, and sharp shooting pains—the latter

¹ No. for October 1847.

sometimes extending to the shoulder and down the arm of the affected side; the former felt only in the swelling, not constantly, but coming on at intervals. The general health of the patient quite good. The catamenia ceased three years ago. She supposes that this breast may have got a blow in the year 1840, when she had a collar-bone broken, and was otherwise injured by the falling of a bridge which she was crossing in a carriage.

The mammary gland and tumour were removed by excision on the 15th of April by Mr Miller. The patient made a rapid recovery, and is now, June 10th, in excellent health.

Examination of the tumour.—Embedded in the fatty tissue of the gland was a firm growth the size of a hen's egg, which on section presented a bluish-gray surface. It was composed of numerous loculi or cysts, the fibrous partitions of which resembled, in some places, those on the surface of a cut orange. They were filled with a pearly, transparent, gum-like fluid, of the consistence of tolerably firm jelly, trembling on the knife. The individual loculi or cysts varied in size from a pin's head to a large pea. The margins of this growth were tolerably circumscribed, a distinct line of demarcation existing between it and the yellow fatty substance of the gland. Here and there a small nodule of the same colloid substance, about the size of a pea, was separated two or three lines from the chief mass, and embedded in fat.

Microscopic Examination.—A section of the morbid growth with a double-bladed knife, exhibited exactly the same fibrous structure, arranged so as to form oval and circular loculi as in the last case, and I have not thought it necessary to figure it again. The contained colloid substance was in some places colourless and structureless, in others loaded with very transparent delicate cells. On squeezing out, or scraping from the surface some of the gelatinous matter, it was seen to contain masses of cells grouped together, as in Fig. 69. The nuclei were generally very apparent, and seemed at first to be embedded in a quantity of granular matter. By careful management of the light, however, the nuclei could be observed to be surrounded with delicate walls compressed together. Some of these groups were loaded with fatty granules, evidently deposited between the nucleus and cell wall. Fig. 69 also represents some of the cells isolated, as in Fig. 68, but more transparent; a few are elongated, approaching the caudate form. Scattered among the transparent colloid matter were masses of fatty granules, often agglutinated together in rows. On the addition of acetic acid, the cells, both isolated and in groups, became more transparent—Fig. 70; and in some of the most transparent gelatinous matter, delicate fibres were formed by precipitation—Fig. 71.

Fig. 69.

Fig. 70.

Fig. 71.

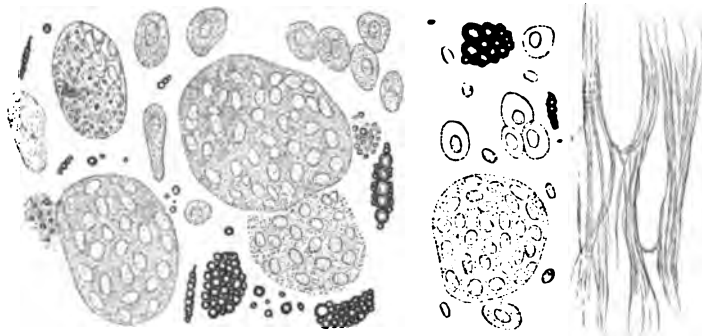


Fig. 69. Cells, isolated and in groups, with granules of fatty matter, squeezed from the colloid cancer described Obs. XXVI. Fig. 70. The same after the addition of acetic acid. Fig. 71. Fine filaments precipitated in colloid matter on the addition of acetic acid.

Remarks.—The colloid cancer in this Observation seems to me to have been in an earlier stage of development than in the two former ones. The groups of cells exactly resembled masses of young epithelium, which led me to examine whether any were attached to the inner surface of the cysts. After making numerous sections, however, with the utmost care, with a sharp double-bladed knife, I could not satisfy myself that the cells bore any relation to the inner surface of the fibrous structure, or that there existed any where an appearance of an epithelium layer. On the contrary, the nuclei were every where scattered throughout the substance of the colloid matter, surrounded by faint cells, and, as we have seen, were easily separated or turned out by pressure, and by washing. The formation of filaments in the colloid matter on the addition of acid, is a fact worthy of observation in this instance. It did not take place in the colloid cancer of the two former observations, but only in the colloid matter in the ovary in Obs. XXIV.

OBSERVATION XXVII.—*Colloid Cancer of the Mamma—Excision—Cure.*

In September 1847, I received from Mr Page of Carlisle a portion of a tumour he had excised from the breast. The following is the account he has been so good as to give me of the case:—"Carlisle, 7th October 1847.—My dear Sir,—The patient from whom the last tumour was removed is a female domestic servant, forty-seven years of age, still menstruating regularly, thin, but not of healthy aspect, but has seldom been the subject of indisposition. A year since she first accidentally discovered a hard tumour, the size of a bean, in the upper part of the left breast, which gradually increased in size, but was not attended with pain until about a month before its removal; during that period, however, she suffered much from occasional attacks of severe lancinating pain. The tumour (September 20th) was the size of a pigeon's egg, hard, and apparently quite distinct from the substance of the breast in which it was imbedded; but when removed it was found to be less circumscribed than had been supposed, as distinct prolongations of the tumour were seen extending chiefly in the direction of the nipple, which was not at all retracted. Of the general structure of the tumour, you can accurately judge from the portion I sent you. The wound healed rapidly; but there is now, less than three weeks from the operation, a very suspicious hardness of one of the axillary glands, at present not larger than half a pea, which is the seat of occasional 'pricking' pain.

From the throbbing pain in the former case, together with the rapid enlargement of the breast, it was for a considerable period looked upon as an abscess; at the time I first saw it, however, the woman suffered comparatively little pain, and that was generally more of a burning than of a lancinating character, although she spoke of occasional severe 'prickings through the breast.'—I am, dear Sir, yours very truly, W. B. PAGE."

In a letter from Mr Page, dated June 6, 1848, he says the cure has been complete up to that date, and she follows her usual occupation of a domestic servant without inconvenience.

Description of the Portions of Tumour received.—I received two portions of the tumour, each about an inch long and broad, and about half an inch deep. They were of a brownish purple colour, composed of a fibrous basis, containing here and there small cysts, the largest not above a line in diameter, filled with a dark brown, semi-transparent colloid matter. In one or two places there were a few gritty particles of calcareous matter.

Microscopic Examination.—A section of the morbid structure presented the same general characters as in the three last cases, namely, a fibrous net-work, forming loculi containing colloid matter. The fibrous element, however, was greater in quantity, and the cysts or open spaces consequently more separated from each other. No isolated cells could any where be discovered; but on squeezing the growth, the contents of the cysts could be removed entire, presenting round or oval masses, as in Observation XXVI. In masses so separated, no nuclei or cells could be seen, even with the most careful management of the light, nor was the existence of an investing membrane at all evident. The whole resembled a finely granular round or oval mass. One of these is figured alone on the left of Fig. 72. On adding acetic acid to a thin section of the tumour, the filaments became more transparent, and the contents of the cysts, while they retained their finely molecular appearance, permitted the existence of distinct nuclei to be observed—Fig. 72. These were of an oval or slightly flattened form, about the 100th of a millimetre in their longest diameter, scattered throughout the substance of the colloid matter. No cell wall was any where to be observed. The calcareous points were composed of irregular semi-crystalline masses, soluble in the mineral acids.

Fig. 72.

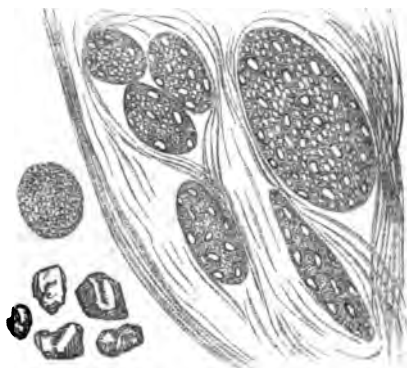


Fig. 72. Appearance of a section of the colloid cancer described Observation XXVII., after the addition of acetic acid. To the left of the figure is seen a round molecular looking mass squeezed from a cyst before acetic acid was added. Below, masses of earthy salts.

Remarks.—In this specimen of the disease, the cell formation evidently presented a less advanced condition than even in the last Observation, and we may fairly suppose that it furnishes us with an example of its earliest development. Nothing but isolated nuclei scattered through a finely molecular blastema was observable, and here, again, no connexion could be made out between these nuclei and the internal surface of the individual cysts.

The four specimens of colloid cancer we have now examined, point out to us that its fibrous element differs materially from that in other forms of cancer, and never presents any traces of having been formed from cell development, or through the agency of nuclei. It seems more probable that they are formed by precipitation, or the coagulation of the albuminous principle originally dissolved in a fluid blastema. The origin of the colloid matter is still shrouded in obscurity. Even for a knowledge of its chemical principles, we

must still look to future research, although it has been supposed to consist of some peculiar form of albumen or fibrin, probably the former. The facts detailed, however, show, that while under some circumstances acetic acid produces little or no effect upon it; at others it causes a copious precipitate, which assumes the form of filaments, which are identical in structure and properties with those forming the basis of the morbid growth. The corpuscles are evidently formed in the colloid matter as in a blastema; first by the formation of nuclei, around which cell walls are formed, which then present all the characters of cancer cells.

In colloid encysted tumours of the ovary, the fibrous structure presents two different arrangements. In one, cyst is formed within cyst; in the other, the whole growth is split up into divisions by partitions—(Fig. 73). In the former case, the cysts are often lined with a distinct epithelium; in the latter, I have not been able to observe this. The colloid matter itself presents the same general characters as those described in connexion with colloid cancer, sometimes forming filaments on the addition of acetic acid, at others not. may be observed of different sizes occurring in groups, in the very centre of colloid masses, as in Fig. 74; at others pale, round, and oval nuclei, as mentioned Obs. XXIV., more or less mingled with delicate filaments—Fig. 75. In some places there are seen faint nucleated bodies—(Fig. 76). In some compound cystic formations of the ovary, the epithelium separates, and the individual cells from endosmosis swell out, and

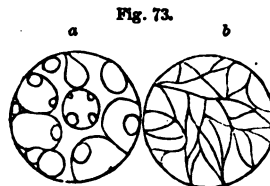


Fig. 73. Two diagrams, showing the arrangement of the fibrous element in ovarian tumours; a, cysts forming within cysts; b, cysts formed by partitions of the fibrous stroma.

Occasionally fatty granules



Fig. 74. Groups of granules found in the centre of colloid masses in the ovary; a, granules of irregular shape; b, granules generally about 1-500th of a millimetre in diameter; c, others half the size.

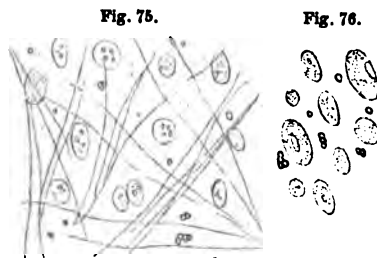


Fig. 75. Round and oval nuclei, with filaments in semi-transparent colloid matter of the ovary. Fig. 76. The same, with delicate oval corpuscles in an early stage of development.

present all the characters of a cancer cell—(Fig. 77). Sometimes these are rendered many-sided from pressure, when their analogy to epithelial cells becomes more apparent—Fig. 79. Careful observation also, will generally enable us to trace their origin to the internal lining of the cyst, as exhibited in Fig. 80.

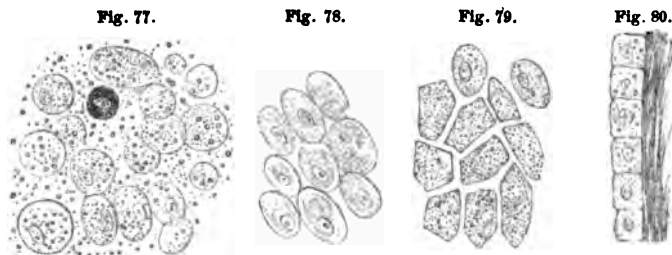


Fig. 77. Well developed cells in colloid matter of the ovary. Fig. 78. The same after the addition of acetic acid. Fig. 79. Similar cells many-sided from compression. Fig. 80. The same cells attached to the internal surface of the fibrous cyst.

ARTICLE II.—*Cases of Hysterocele; with Remarks.* By JOSEPH BELL, Member of the Faculty of Physicians and Surgeons of Glasgow, Lecturer on Botany, Anderson's University.

(Read at a Meeting of the Glasgow Medical Society, March 1848.)

HERNIA of the uterus is a very rare affection; few cases have been recorded. It seems to occur under three forms:—

- 1st, The protrusion taking place through the inguinal canal.
- 2d, The outlet being the crural opening.
- 3d, A separation or rent in the abdominal walls.

Two cases of the last kind of hysterocele having come under my notice, I have taken the present opportunity of bringing the subject before this meeting of the Society.

CASE I.—June 2, 1843. I was requested to visit Mrs P——, who had been eight days previously delivered of her fifth child. I found her much exhausted; pulse quick and small. She complained of intense pain of abdomen, on examining which the uterus was found protruding through a rent in the linea alba, extending from pubis to near ensiform cartilage. A portion of intestines was also protruded, but these were easily reduced; with the uterus, however, more difficulty was experienced. Compresses and bandages were applied, but had to be removed in consequence of the aggravation of pain which the pressure caused.

The patient died on the 4th instant. No post-mortem inspection was permitted. She was a tall slender person, of a very relaxed frame; so far as I could ascertain, the hernia had no existence previous to her last confinement.

CASE II.—On the 5th of May 1846, I attended Mrs M—— at her fourth confinement. At five o'clock A.M., after a tolerably severe labour of sixteen hours' duration, a fine female child was born. As soon as the cord was separated and the child handed to the nurse, in accordance with my usual practice I applied my hand to the abdomen, when, to my astonishment, I found the anterior

superior part of the uterus protruded through a rent in the linea alba, which was completely torn through from the ensiform cartilage to pubis. The uterus felt so large and firm, that little doubt existed of its containing another child.

On making a vaginal examination a second set of membranes was detected, on rupturing which the child was ascertained to present with nates to mother's abdomen. The uterus was pressed back into abdominal cavity, and firmly maintained in this position with both hands until the child (a male) was born; this occurred half an hour after the first birth.

The conjoined placenta came away immediately—the uterus contracted firmly, and descended into the hypogastrium.

A compress was placed on each side of the rent along its whole extent, and a bandage firmly applied.

A smart attack of peritonitis occurred, but it was fortunately subdued by the ordinary treatment. Three months after delivery I carefully examined the abdomen, and could detect no trace of the rupture.

This woman is of small stature. The great distension to which the abdominal parietes were subjected from the uterus containing two very large children, must have stretched the fibres of the linea alba to their utmost extent, the contraction of the abdominal muscles during labour producing the complete separation of the tissues. The only circumstance which I noticed attending the rupture, was, that, almost immediately before birth of first child, the patient complained of a burning pain in the abdomen and a sensation of faintness; but as the child's head was at this time partly protruded through os externum, and the complaint being made during the pain by which the head was wholly expelled, no attention was given to her statement.

I find a case related by Boivin and Dugés very similar to that of Mrs M——.

A woman who had been in labour three days, experienced on a sudden violent pain, with a sense of laceration in the abdomen, followed by extreme weakness. J. L. Petit found, on examination, a ventral rent or rupture extending from the umbilicus as far as the pubis, and another from the umbilicus to the ensiform cartilage. The lower one was so considerable that the recti muscles were separated from each other by a distance of nine or ten inches, allowing the uterus to completely protrude.

There are recorded several cases in which the hysterocele has taken place through a ruptured cicatrix. For example, Ruysch met with a case of a woman who had been the subject of a large abscess followed by a cicatrix just above the pubis, having become pregnant the cicatrix gave way, allowing the uterus to protrude.

Rousset states, that he has seen the same occurrence after a first Caesarian section.

Dr Ashwell, in his excellent work on the diseases of women, mentions a similar case.

Instances of inguinal and crural hysterocele are related by Senner, Doringius, Lallemand, Chopart, and Murat; but not having observed any of these forms I shall not enter into details regarding them.

The treatment in cases of ventral hernia is very obvious,—replace-

ment of the uterus, compression, and bandaging, with confinement to the recumbent position for weeks. Indeed, in every form of hysterocoele reduction ought to be practised if possible. In cases of pregnancy, such as that related by Dr Ashwell, the treatment which he and Dr Butler adopted was successful. The uterus was raised up, supported firmly with the hands, and pressed backwards into abdominal cavity. In a case of the inguinal variety, Saxtorph merely raised up the fundus uteri and labour was terminated favourably, but the uterus remained permanently protruded; no means, however, were taken to effect reduction after birth of child.

Boivin and Dugés recommend the Cæsarian section to be performed, if the raising of the fundus does not allow delivery to take place. They inform us, that this succeeded in the hands of Rousset in a case where the labour afterwards took place spontaneously, notwithstanding a considerable increase in the size of the hernia.

In the event of strangulation, Davis, Boivin, and Dugés recommend immediate recourse to the proper operation for the removal of the stricture.

ARTICLE III.—*Contributions to the Pathology of the Kidney.* By
WILLIAM T. GAIRDNER, M.D.

PART I.—PATHOLOGICAL ANATOMY. (Continued from p. 865.)

II.—*Lesions affecting chiefly the Vascular System of the Kidney.*

IN passing to the consideration of the morbid changes which occur in the vascular system of the kidney, the conditions of sanguineous congestion and extravasation on the one hand, and anæmia on the other, would fall to be described first in order. But the simple hyperemic and anemic states of the organ have been noticed so fully in the anatomical introduction (April No. p. 716,) that little more remains to be said on this subject.

Congestion followed by permanent obliteration of the Capillaries of the Cortical Substance.—Under this head I have to describe a form of lesion in the kidneys which, although certainly of less frequent occurrence than those characterised by exudation, is of a pathological and practical interest in no way inferior to any other.

The appearances most characteristic to the naked eye of this form of lesion, are those so admirably figured and described by Rayer as the second form of his "*néphrite albumineuse*." The kidneys are generally increased in size, sometimes very remarkably so. Their consistence varies; they are sometimes more flaccid than in the natural condition, but always preserve considerable tenacity. The surface is either quite smooth, or more or less depressed and furrowed. The venous vascularity assumes to a considerable extent the stellate form; the polygons are mostly absent; and the extreme

irregularity and abruptness of distribution of the superficial veins gives to the surface a variegated or "marbled" appearance, which is quite characteristic of this stage of the affection. (See Rayer, Plate VI. Figs. 2, 3, 5; Bright, Plate II. Fig. 1.) Occasionally, also, amid this unequal injection there are to be found scattered petechiæ, indicating recent extravasations of blood into the tubes. On section the cortical substance has considerable volume, and presents a smooth, glistening, almost semi-transparent appearance, which cannot be better distinguished than by the term *waxy*. It may partake in a slighter degree of the variegated character of the surface; more commonly it is of uniform appearance, and of a yellowish or fawn-colour, sometimes verging into a pale flesh tint. The vascular striæ of the cortical substance are generally to be traced by a more or less distinct injection, and a few injected Malpighian bodies, or petechiæ of extravasation, are sometimes dispersed through the section. (See Rayer, Plate X. Fig. 3.) In other cases a little further advanced, both the striæ and the Malpighian bodies are nearly destitute of blood. (Rayer, Plate X. Fig. 1; Bright, Plate II. Fig. 1.) The pyramids frequently retain their normal vascularity; sometimes, however, they are of a pale colour, and their bases are indistinctly marked,—a condition which indicates the progress towards a further disorganization.

When a kidney in this condition is carefully and minutely injected, the greater proportion of the cortical substance remains impervious; the injection, however, can frequently be made to penetrate as far as the cortical striæ, and even to some of the Malpighian bodies. (See Rayer, Plate X. Fig. 2; Bright, Plate II. Fig. 3.)

From these circumstances it is obvious, that the lesion above described consists in an obliteration or obstruction of the capillary system of vessels throughout the organ, and a partial obliteration of the veins on its surface. There is also every probability that this condition is secondary to one in which there is a high degree of congestion of the organ. The extravasations, the occasionally injected Malpighian bodies, and the highly injected, though partially distributed, stellar veins, leave no doubt that the state of congestion described as the first form of albuminous nephritis by Rayer, is really the antecedent of the present or second form.

To any one who is familiar with the *marbled* and *waxy* kidney here described, there can be no difficulty in recognising a further stage of the same lesion, in which the organ is perfectly pale both on the surface and on section, with the exception, perhaps, of a very few stellated superficial veins. The kidney in this stage (the transition to which seems to be represented in Rayer, Plate VI. Fig. 4) is still heavy and voluminous; it acquires additional firmness and elasticity, and assumes much of the general appearance of a true non-vascular texture. It varies from a light yellow to a fawn-colour, which extends to the pyramids, the bases of which become still more confused and intermingled with the cortical substance than in the

marbled kidney. The capsule is frequently more firmly adherent to the external surface than in health.

From the pale and yellow appearance of the kidney in this stage, it is very apt to be mistaken, even by a practised eye, for an extreme degree of the fatty degeneration. A well-marked example, indeed, will hardly give rise to this error, if attention be directed to the degree of firmness of the organ, the peculiar lustrous character of the cut surface, and the entire absence of the opaque granulations of Bright, or of that dull tint which distinguishes the excessive degrees of the fatty disease. The appreciation of these characters is, however, more difficult where, as sometimes happens, exudation is also present; and the distinction which has escaped the acute observation of M. Rayer, has undoubtedly been overlooked by many other observers.¹

The microscopic characters of this lesion are chiefly negative. There is not unfrequently an entire absence of exudation; indeed, in the most marked cases of the lesion, I have seldom found even the slightest trace of any abnormal deposit. Occasionally, however, there is a very minute quantity of fatty exudation in the tubes, generally in very small granules, and scattered throughout the organ. The tubes are either natural, or in the advanced stages pass into some of the states hereafter to be described. The capillary vessels surrounding the tubes are not visible, and in their place there is fibrous tissue, which in this form of lesion always appears somewhat exaggerated. The Malpighian bodies are also frequently seen in process of obliteration, and surrounded by dense capsules of fibrous tissue. The epithelium is frequently altered in character, but its changes follow no fixed rule.

The absence or scantiness of exudation, taken in connexion with the extent of degeneration appreciable by the naked eye, are amply sufficient characters to distinguish this lesion from the extreme stages of the fatty disease.

I shall add three observations which illustrate the different stages and varieties of this important form of renal degeneration. I do not at present mean to enter on a consideration of the symptoms, further than to say that both Bright and Rayer have figured it as being connected with albuminous urine and dropsy; and these facts entirely concur with some of my own observations. This lesion is therefore undoubtedly one form of what is commonly called Bright's disease.

OBSERVATION XI.—*Marbled and Waxy Kidneys* (without exudation) *Bronchitis—Large Abscess in Labium*.—A woman was admitted into the Royal Infirmary in a state of great exhaustion from an abscess, the size of a child's

¹ Plate VI. Fig. 4 of Rayer's work, is probably an example of the waxy pale kidney; Plate VII. Figs. 2, 3, 4, of the fatty disease. The distinction is sufficiently evident even in the plate. All of these are referred by Rayer to the third form of "Néphrite Albumineuse."

head, connected with the external parts of generation. This was opened, but a few days after admission she died.

On dissection, November 28, 1847, the lungs were found much engorged, and the bronchi full of fluid. The kidneys were greatly enlarged, and weighed $8\frac{1}{2}$ and $6\frac{1}{2}$ oz. They were nearly of the usual consistence. The surface was very uneven, from being marked all over with irregular depressions and furrows. There was a considerable amount of venous injection, but very irregularly distributed; so much so, that the surface, which was at some parts quite bloodless, had an irregularly variegated appearance (not unlike the case of Sallaway, in Dr Bright's work, Pl. II. Fig. 1.) The depressions in the surface were mostly filled with stellated veins. On section, the cortical substance was of increased volume, particularly between the pyramids; these were broad at the bases, and the line of separation from the cortical substance was quite distinct. The pyramids were well injected; the cortical substance contained, in the line of its striae, a few points of unusually distinct injection, but was generally anemic, being of an exceedingly pale and clear salmon-colour. The surface of the section was very smooth, and neither the section nor the surface presented the slightest trace of granulations. The mucous membrane of the pelvis of both kidneys was deeply injected with arborescent vessels. The renal veins were distended with dark blood.

On examination by the microscope, a few injected Malpighian vessels were seen; the majority were bloodless. The capillaries were uninjected. The normal epithelial cells were in great abundance; but neither in these, nor in the tubes, could any fatty granules be observed, although numerous sections were made for the purpose of determining this point.

The remarkable similarity in appearance of the section of this kidney to that in Rayer, Pl. X. Fig. 3, was noticed at the time. Both of them may be considered as good and characteristic examples of this affection, in a moderately early stage. The marks of recent vascular excitement were observable in the irregular injection of the surface, the congested state of the pelvis of the kidney, and the points of injection in the cut surface. On the other hand, the absence of exudation, and the smooth waxy appearance of the section, distinguished it from the fatty kidney. The increase in size and weight must have been owing to the great abundance of secreting epithelium within the tubes.

OBSERVATION XII. *Mottled Waxy Kidneys* (with slight exudation)—*Fatty Liver—Softened Spleen—Fever and Scurvy*.—Daniel Kean, æt. twenty-three, was admitted May 19, 1848, into a fever ward. In addition to the ordinary symptoms of eruptive typhus, he suffered from painful induration and ecchymosis in the calves of both legs, with spongy and ulcerated gums (symptoms in every way similar to those of the endemic scurvy of last year). For some time before admission he had lived very poorly, chiefly on bread and coffee. He died on June 6th.

On dissection, the lungs were congested, the heart and liver paler than usual, the spleen very soft, and slightly enlarged. The kidneys were slightly above the normal size, their capsules more firmly adherent than usual. Their surface was rendered uneven by the presence of shallow grooves and irregular dimples. The venous polygons were very obscure, being mostly supplanted by stellated and arborescent vessels, which were so distributed as to give the surface an irregularly mottled appearance. Over different parts of the surface were scattered petechiæ, varying from a dark purple to a slate colour. On section, the cortical substance appeared generally of a somewhat bright fawn

colour. The cut surface was perfectly smooth, and had a waxy lustre. Here and there were seen points and lines of injection, corresponding with the vascular striae of the cortical substance; also a few larger and deeper-coloured petechiae. The whole of the injection was very irregularly distributed. The pyramids were paler than usual, and the line of demarcation with the cortical substance was slightly irregular. The pelvis and calyces were normal. At one point of the cortical substance, in the left kidney, there was found a patch of rather light yellowish colour, about the size of a pea, which was surrounded by a very distinct and abrupt rose-coloured border; this, on minute examination, was obviously composed of highly injected Malpighian bodies.

On examination by the microscope, the fibres of the heart were found in most places occupied by small granules, which, however, did not generally completely obscure the striae. The liver contained oil-globules in considerable numbers, some free, and others within the epithelium cells. In the tubes of the kidneys there could be seen in many parts of the cortical substance a few minute and scattered granules, but they were mostly filled by perfectly clear cells and nuclei, which were in preternatural abundance. The nuclei were mostly smaller than usual. The inter-tubular fibrous tissue appeared unusually dense, and no injected capillaries could be seen. A few Malpighian bodies were partially injected. In the part above described as surrounded by an injected border, the tubes and cells contained granular exudation in a quantity much more considerable than elsewhere.

In this kidney, we have the waxy degeneration accompanied by a certain degree of exudation, although not to such an extent as to give any special character to the lesion. The petechiae are interesting in connexion with the scorbutic diathesis which existed; nevertheless, the state of the superficial veins, and the injected state of some of the Malpighian bodies, along with the fact of extravasation into the tubes, appear to indicate, as in the former case, a recent condition of vascular excitement. Whether the exudation occurred as a consequence of this condition, or of the general tendency to fatty degeneration, as shown in the liver and heart, is open to question. The former view appears, from the character and partial distribution of the exudation, to be not improbable.

The succeeding case is an example of the more confirmed form of this degeneration.

OBSERVATION XIII.—Pale yellow waxy Kidney (without exudation)—Enlargement of Liver and Spleen—Bronchial dilatation—Pneumonia.—Isabella M'Kinlay, æt. twenty-three, admitted November 15th, 1847, into Ward 15 as labouring under fever. She was weak and emaciated, suffering from severe dyspnoea and cough, with fever and pain of the left side of the chest. Her complaints were of long standing, and she had had occasional diarrhoea. There were distinct physical signs of cavities in the left lung, and of general bronchitis. She was supported by wine, but continued in a febrile state, with much depression, and slight jaundice, and died December 1st.

On dissection, the heart was pale, but of natural size. In the left lung, which was much diminished in size, there were numerous large cavities. The vesicular structure of the lung was wholly obliterated, and the cavities were found to consist of dilated bronchi, the walls of which were much hypertrophied. The right lung was slightly condensed in the upper part. The liver was enlarged, pale, and firm. The spleen was large and soft. The kidneys weighed 9 oz. and 8 oz. The capsule adhered with unusual firmness. The surface was uneven and furrowed, at some points roe-like or botryoidal; but the projections here indicated had not the opacity and whiteness characteristic of the

granulations of Bright (for an illustration of this point, see Rayer, Pl. X. Fig. 10). The surface was perfectly pale, with the exception of a very few straggling arborescent veins. The whole cortical substance was of a pale straw colour, tumid, and perfectly anemic; the pyramids were very slightly mottled from vascular injection, and appeared to present straggling radiations, which were prolonged indefinitely into the cortical substance towards the surface; the bases of the pyramids were thus rendered very indistinct. The whole kidney was of much firmer consistence than usual. Pelvis and calyces normal.

The principal microscopic appearances were dilatation of the tubes, and entire absence of all appearance of vessels. The epithelium was in considerable abundance, and had a tendency to cohere in masses, bearing the form of the tubes. Many of the cells were also compressed. A doubtful trace of granular exudation was here and there observed; but, after a most careful search, nothing very definite could be seen.

Some of the alterations in this kidney will be considered under the head of lesions of the tubes and epithelium. At present it is sufficient to say, that this case was considered by many persons who were present to be one of fatty degeneration of the kidneys, as well as of the liver and heart. Microscopic examination proved, however, that none of these lesions existed in any appreciable degree; the paleness of the heart and liver being, I believe, the result of anæmia, while that of the kidneys was the indication of an advanced stage of the waxy degeneration.

Connexion of Congestion with the Process of Exudation.—I have already stated, that in general the degree of fulness of the vessels stands in an inverse ratio to that of the tubes; and that accordingly an amount of exudation, so great as to produce distension of the tubes, generally produces a corresponding depletion of the vascular system. I have likewise shown, by reference to the works of Rayer and Bright, as well as by original observations, that paleness of the cortical substance is one of the most frequent characters of kidneys containing exudation.

A moderate or small amount of exudation, however, sometimes occurs in organs which present considerable vascular injection, and sometimes even a distinctly hyperemic condition. Such cases occur not unfrequently in the latter stages of heart disease. In only one or two cases have I found the vascularity much greater than usual; in these the kidneys were of large size (weighing from six to eight oz.), and contained very little exudation. In one instance they presented on the surface the petechiæ indicative of extravasation into the tubes (as in Bright's fifth Plate); in this case there had been albuminous urine and dropsy during life, with obstinate hæmaturia during two months before death; there was also intense dysentery and disease of the liver. The kidneys were otherwise diseased; but the exudation was in small quantity, and in very minute granules. In another case (a woman who died of convulsions in the eighth month of pregnancy, and in whom an apoplectic clot was found in brain), the kidneys weighed four and four and a half oz.;

they were firm in texture, and of a tawny orange colour, pretty generally diffused. This was found to proceed from the presence of blood in the tubes, intermixed with small fatty granules, which were in considerable abundance. There was, however, little capillary injection, and the Malpighian bodies were mostly bloodless. The urine in this case had not been examined.

The analogy between the results of exudation in the kidney, and those in the lung and other organs, would naturally lead to the idea, that, as in the red hepatization of the lung, the deposition of granular exudation is preceded and accompanied by vascular turgescence, so in the kidney there may be a form of the exudative process in which active congestion of the organ plays a part. Almost all pathologists since the time of Dr Bright, who have written on the diseases of the kidney, have dwelt more or less strongly on the forms of renal disease, accompanied by acute symptoms and a congested state of the organ, as being the precursors, in some instances, of the more ordinary changes, in which no increased vascularity occurs; and Bright himself has indicated "an inflammatory state of the kidney" as a probable cause of the structural changes which he described and classified.—(See Bright's *Reports*, Vol. I. p. 3). This remark is strongly borne out by the case of Evans (p. 33), to which Pl. V. refers. Christison and Rayer concur in describing a state of congestion as the commencement of many of the chronic changes, and have established the coincidence of such a state, in some instances, with symptoms of acute dropsy and albuminuria. Even supposing the opinions of these authors as to the connexion between the hyperemic and anemic changes to be, as they probably are, founded partly on theoretical views as well as on direct observation, they must be admitted to be of great weight in a question which requires for its solution so large a field of experience.

From the comparatively small number of cases of acute disease of the kidney, and more especially of acute dropsy, which have occurred in the Edinburgh Royal Infirmary since my attention has been turned to this subject, I am unable to add any observations bearing more directly upon this question than those to which I have already alluded. Whether, in the two cases above mentioned, the evidences of vascular excitement, and the exudation in the tubes, can be considered as any thing more than accidental concomitant circumstances, I am not prepared to say without further opportunities of investigation; but if these cases can be considered as examples of a state of the kidney analogous to red hepatization of the pulmonary texture, I am satisfied that this state is of much rarer occurrence, or at least more rarely fatal, in the former organ than in the latter.

I have alluded incidentally to Dr Johnson's views on this subject. He considers the deposit of fatty granules in the kidney as being invariably a chronic process, never preceded, in any case, by congestion or extravasation. On the other hand, admitting that these

states are frequently found in connexion with fatty exudation, he considers them as secondary results of the distension of the tubuli uriniferi.—(*Med. Chirurg. Transactions*, Vol. XXIX, pp. 4, 8, 9.) I have already shown (*May* No., p. 796), that this view is opposed both by anatomical considerations and by pathological facts; and, in particular, that the confirmed fatty degeneration of the kidney is usually accompanied by nearly complete depletion of the vessels. The comparative rarity of congestion in connexion with fatty exudation, under all circumstances, and its greater frequency while the exudation is still small in amount, sufficiently show the inadequacy of Dr Johnson's hypothesis to explain the phenomenon. The assertion of the exclusively chronic nature of the deposit in the kidney, is probably founded on an imperfect theoretical view of the nature and origin of fatty exudations in general.—(See *May* No., pp. 803-5.)

On the whole, the supposition which appears to harmonize best with the analogies of other organs, and also with what has been hitherto observed in the kidney, is, that the oleo-albuminous or fatty exudation is sometimes preceded and accompanied by a congestive stage of short duration, in the course of which extravasation of blood into the tubes may occur. When, however, the exudation has accumulated within the tubes to such an extent as to cause fluid pressure by obstruction, the vascular system of the organ is emptied of its blood in a degree proportionate to the amount of distension; and, as the exudation continues to increase, the stage of congestion is rapidly superseded by the development of the pale yellowish colour so frequently mentioned in connexion with the fatty degeneration. This view appears to be strongly supported by the cases formerly mentioned, in which the oleo-albuminous exudation occurs in scattered whitish patches, surrounded by a distinct vascular rim.—(*June* No., p. 864.) On examining microscopically a section of such patches, I have observed the line of demarcation between the congested and the depleted Malpighian bodies to correspond accurately with the boundary of the exudation, so as to render it probable that the congestion, originally present throughout the diseased portion, had been superseded by the presence of the abnormal deposit.

Whether any connexion exists between the development of the congestive form of exudation and the presence of acute symptoms, is a question for further clinical experience, united with careful pathological investigation, to determine. As it is evident that the earlier stages of disease in the kidney have been hitherto to a great extent overlooked, both at the bedside and in the dissecting-room, it is highly probable that many of those affections which have been considered as most obviously chronic in their nature, may in reality be the advanced stages of processes more or less acute, which have not been fatal in the first instance, or which, if fatal, have not presented lesions appreciable by the unaided eye.—(*To be continued.*)

ARTICLE IV.—*On a New Galvanic Battery.* By THOMAS WRIGHT, M.D., Fellow of the Royal College of Physicians, Edinburgh.

THE voltaic battery, as an agent in the treatment of disease, has for some time been superseded by the electro-magnetic coil machine; and it has been rather too hastily concluded that the physiological action of the two instruments is identical. The electro-magnetic coil machine, even in its most improved form, is only capable of giving a succession of shocks and sparks resembling repeated discharges from a large Leyden jar weakly charged; whereas, in the action of the voltaic battery, three distinct effects may be readily observed:—first, a *shock* both on making and breaking contact with the battery, succeeded, with some interval, by the second effect, a *momentary sensation of strong heat*; and thirdly, the *continued passage of the voltaic current*, which, as Matteucci has shown, tends to depress or exalt the energy of the nerve as it is passed in the same or in the contrary direction to that in which the nervous influence is naturally transmitted. The above considerations have led me to give considerable attention to the improvement of the battery, and to the construction of the arrangement I proceed to describe.

Having left a Smee's battery (platinized silver and zinc) in a very strong solution of sulphuric acid, I found, on attempting to use it the next day, that a great part of the zinc was dissolved, and the electrolyte saturated with the sulphate. On transferring the plates to a vessel of fresh dilute acid, a considerable quantity of hydrogen continued to be evolved from the platinized plate, although the poles of the arrangement were unconnected; and a further examination showed that a crust of anhydrous (?) sulphate of zinc had extended itself between the plates, which, possessing a low conducting power, had closed the circuit as effectually as a fine copper wire could have done. It instantly suggested itself to me, that a plate having a coat of this salt might be rendered available as the negative plate in a voltaic arrangement, and that a battery might be thus formed of one metal, zinc, alone. After a long series of experiments, which I need not detail, it was found that sheet zinc continued inactive in a mixture of nitric and sulphuric acids, in the proportion of one part of the former to five of the latter; and that a battery formed of a porous cell, including a plate of zinc in the above mixture, surrounded by another zinc plate immersed in a saturated solution of common salt, possessed considerable electro-motive power, but not nearly equal to what I had calculated on. The negative zinc plate was now removed, and, after having been well washed and brushed over with a solution of chloride of platinum, again washed, dried over a gas flame, and replaced in the porous cell. This arrangement (platinized zinc and zinc) formed a most powerful battery; a single cell, the platinized zinc of which was only two and a half inches square, gave most brilliant sparks and intense

shocks with an electro-magnetic coil, and, after the plates had continued immersed more than five hours, was found not to have declined in power. The same arrangement was also found to have the power of decomposing water, with elimination of both the gases; and a small compound battery of twelve pairs, the porous cells¹ of which were each thrust into a liqueur glass of moistened salt, and the arrangement completed by arcs of zinc, one half of each of which was platinized, gave shocks of considerable intensity. A zinc plate covered with plumbago was found to act well on first immersion, but soon declined, the coating having floated away into the solution. Copper and brass, which were also inactive in the acid mixture, developed a current more powerful than *unplatinized* zinc. A plate of sheet iron gave good results; this metal, which in concentrated nitric acid alone is inactive only for a short time, continues so in the nitro-sulphuric acid mixture.

The advantage of the platinized zinc battery is its simplicity of construction, the troublesome adjuncts of binding screws or mercury cups being unnecessary. Where several pairs are employed, the plates are bent into arcs, in such a form that the zinc, after leaving the solution of salt in one cell, rests with its platinized leg in the porous vessel of the next. The zinc, which need not be thicker than a card, is easily cut into proper shape with a pair of common scissors, and moulded on the porous troughs.

Since writing the above, I find that zinc is also inactive in a mixture consisting of water three parts, nitric acid one part, and sulphuric acid five parts, and that a pair of plates simply immersed in this solution, without the intervention of a porous cell, is capable of exciting a voltaic current, which I think will prove one of considerable energy. Care must be taken that the mixed acids are allowed to cool before use.

ARTICLE V.—*On the Specific Gravity of Chloroform, and its Superiority when Pure as an Anæsthetic.* By GEORGE WILSON, M.D., Lecturer on Chemistry.

THE object of the following communication is to make known to practical chemists and to medical men, that much of the chloroform at present in use is not furnished by the manufacturer in such a condition of concentration and purity as is essential to its producing its maximum effect as an anæsthetic; whilst, on the other hand, there is no difficulty, so far as either trouble or cost is concerned, in supplying it of great strength and purity.

Two of the physical characters of chloroform, viz. its density and

¹ The porous cells in the arrangement were formed of the bowls of tobacco pipes.

its odour, furnish convenient approximative tests of its purity; so also does its physiological action, which is sensibly altered by the presence of even small quantities of foreign matters.

First, of its density. This is given in the text-books as 1.480; but I believe that it has not escaped the notice of almost any of the manufacturers of chloroform, that its specific gravity is considerably higher than the number stated above.

In the course of some purely physical and chemical researches, carried on by myself and my friend Mr William Swan, it became essential to know what was the density of pure chloroform. For this purpose I carefully rectified two specimens of the liquid, and as their specific gravities proved to be much above 1.490, I thought it desirable, from its importance to medicine, to ascertain the densities of specimens of chloroform prepared by different makers, and purified in different ways.

The following table gives the specific gravities of nine specimens. Those marked commercial were examined as they were furnished by four different manufacturers. Those distinguished as washed and rectified, were commercial specimens which were agitated with water, and afterwards rendered anhydrous by being left for a night over chloride of calcium and subsequently distilled. One specimen was washed but not rectified.

The densities of these chloroforms were determined by Mr Swan, with a balance in my possession, weighing delicately to the thousandth of a grain. They are stated in the order of their density:—

Specific Gravity of Chloroform at 60° F.

1. Commercial,	1.4636	6. Washed and rectified,	1.4966
2. Commercial,	1.4851	7. Washed,	1.4968
3. Washed and rectified,	1.4929	8. Commercial,	1.4974
4. Washed and rectified,	1.4937	9. Washed and rectified,	1.4980
5. Commercial,	1.4939		

The mean of these numbers is 1.4909. The first specimen, however, must be excluded as of much too low density for pure chloroform. The second, too, is so much below all that follow it, that it must be set aside also. The mean of the numbers for the remaining seven specimens is 1.4956. One of these, however (7), had been purposely saturated with water, and could not therefore have the density of pure anhydrous chloroform. The specimens numbered 8 and 9, are those on which most reliance is to be placed. A fluid ounce of the former evaporated to dryness, left no residue, nor could any impurity be detected in it. The latter I washed and rectified very carefully. It was ultimately distilled twice: once from chloride of calcium, and a second time in a dry retort, to remove any trace of that salt which might have passed over in the first distillation. The mean of Nos. 8 and 9 is 1.4977.

From these results, it appears that pure chloroform, instead of having a density of 1.480, should have one of at least 1.495, and

may be prepared, as it is indeed by at least one maker in town, with a sp. gr. of 1.497.¹

A greater density, then, than has hitherto been required, should be demanded in the case of chloroform. A high specific gravity, however, is not enough to warrant the purity of this liquid. Specimens with the density 1.494, do not in many cases produce the full and pure anæsthetic effect of chloroform containing no foreign matter.

The presence of impurity in such cases can frequently be detected by reference to the second character of good chloroform already alluded to; viz., its odour. It should possess, according to Dr Christison's apt description, "a peculiar fragrant, ethereal, apple-like odour." Specimens, however, which act imperfectly as anæsthetics, are generally wanting in the fragrance of the pure liquid, and exhibit, on the other hand, the ethereal fruity smell, modified by a sharp odour (or flavour), probably occasioned by some acid or irritating volatile compound.

I have not had the opportunity of observing, except on the most limited scale, the physiological action of this substance, so that I do not presume to make statements on my own authority in reference to the relative anæsthetic powers of pure and impure chloroform; but I was led incidentally to notice, what I believe is familiar to all who have made considerable medicinal use of it, viz., that a very small amount of impurity alters the physiological action of this body; and further, what is not I believe so generally known, that the impurities contained in commercial chloroform may be removed by a very simple process, and its anæsthetic qualities thereby greatly increased.

A relative having occasion to have a tooth extracted, I requested Mr Nasmyth to use the chloroform which I had washed and rectified, as mentioned above in reference to the specimen numbered 6, of which it was a portion. Mr Nasmyth was so struck with the unusual rapidity with which anæsthesia was induced by this chloroform, that I left a portion of it with him, that he might ascertain whether it exhibited the same superiority when used with other patients, as the quick result in a solitary case might be the result only of individual peculiarity of temperament or constitution. In further trials, however, it maintained its superiority so unequivocally, that, at Mr Nasmyth's request, I washed, and rectified from chloride of calcium, a considerable quantity of commercial chloroform. On recently applying to this gentleman to ascertain from him what estimate he now formed of the relative anæsthetic powers of the ordinary and the purified chloroform, he stated to me, in reference to the latter, "I am quite satisfied that it is superior to any other kind which I have used. A smaller quantity produces the anæsthetic effect, and it is not so liable to cause coughing or other irritation.

¹ I have been informed that one retail druggist in London advertises chloroform of sp. gr. 1.500. This number has been probably obtained by using an ordinary balance, or the hydrometer, or the hydrostatic beads, none of which, as generally constructed, would distinguish between the densities 1.498 and 1.500.

Probably avoiding the disturbance caused by the irritating cough, allows the chloroform to take effect more readily, and a smaller quantity, in consequence, answers the purpose."

The density of the rectified chloroform, which Mr Nasmyth first employed, was 1.496, a higher sp. gr., as already mentioned, than that of any of the commercial specimens examined, except one, which was quite pure.

A medical journal is not the place for speculating on the cause of so much of the chloroform offered for sale being impure. Nor, indeed, would it be easy to find data for such a discussion, even if it were admissible; for different manufacturers follow different processes in purifying the substance, and few of them are communicative as to their methods. My chief object at present is to point out to medical men, that the anæsthetic properties of chloroform may be much increased by simply agitating it with water, and then drawing it off. Rectification from pure oil of vitriol, or chloride of calcium, must further be practised, if the substance be wished in a state of absolute purity.

If chloroform, however, be freed from all other impurities, the rendering of it anhydrous may, I believe, be dispensed with. The amount of water which this substance can dissolve, is much too small to affect sensibly its physiological action. In proof of this, I mention the following result:—A specimen of chloroform was divided into two portions. The one was washed and rectified, the other simply washed, and after it had been well agitated with a large volume of water, and left for a night under it, drawn off from beneath the lighter liquid. The rectified chloroform had the sp. gr. 1.4980, the washed portion the density 1.4968. The difference here is very small, and the result shows that pure chloroform may be saturated with water, and yet show a density of 1.496, which is above that of much of the so-called anhydrous chloroform of the shops.

Two thousand parts of water are necessary to dissolve one of chloroform (Christison). The loss, therefore, in washing cannot be great, and, as Mr Nasmyth reports that less of the pure chloroform suffices to produce its characteristic effects, it may be questioned whether the washing will not effect a clear gain to the medical man.

The observation of this fact led me to hope that distillation and rectification might be dispensed with in the preparation of chloroform, and the price of that substance lowered. Messrs T. and H. Smith of Duke Street, who make chloroform of great purity, were kind enough to try a modified process at my request, but circumstances have prevented me examining the results, and any reference to it must be reserved to another occasion.

ARTICLE VI.—*Case of Acute Hepatic Abscess in a Man eighty years of age; Evacuation by the Lancet; Recovery.* By WILLIAM M'LEAN, Surgeon, Kilmalcolm.

March 8th, 1847.—I was requested to visit Mr R. Taylor, miller, æt. eighty, a stout and athletic individual, who had been affected with rigors, alternating

with a hot and dry skin, followed by profuse perspiration. These symptoms were now accompanied by obtuse pain in the right hypochondrium, and stretching upwards to behind the right shoulder. There was great tenderness and tumefaction in the hepatic region; pulse was 120, strong and bounding; appetite impaired; thirst urgent; tongue covered with a dark-brown fur; bowels constipated; urine of a dark wine colour, and depositing, on cooling, a bile-coloured sediment. *He was bled to thirty oz.; twenty leeches were applied to the hepatic region, and he was ordered a purgative powder of calomel and jalap.*

9th.—Had a restless night from severity of pain; febrile symptoms still urgent; pulse 110, wiry. The leeches bled freely; blood obtained from the arm buffed. The powder operated well; dejections bilious and very fetid. Swelling in right hypochondrium much increased, and occupying the entire extent of the liver. *A blister to be applied to the hepatic region, and repeat purgative powder.*

11th, Slept badly last night, having had several rigors. To-day the pulse is 100, and wiry; the tongue is cleaner and the thirst less urgent. The pain in the hepatic region is less severe; the swelling is more diffused, and presents obscure fluctuation. The blister rose well, and is discharging freely; the powder procured four large fetid bilious stools. *He was bled again to twenty oz.; a poultice was directed to be constantly applied to the right hypochondrium, and he was ordered a morphia draught at bed-time.*

14th, Tumefaction much increased, with evident fluctuation. An incision was made with an abscess lancet, and four saucerfuls of well-formed pus tinged with bile were evacuated. *A seton cord was inserted into the wound, and the poultice was ordered to be continued; morphia draught to be repeated.*

15th, A large quantity of matter was discharged during the night, and on withdrawing the seton to-day as much pus mixed with bile was evacuated as at the time of incision. Passed a bad night; pulse eighty, soft; thirst much abated. *Quinine with a little wine was prescribed; and to have beef tea for dinner.*

From this date the swelling and discharge diminished gradually, and the general symptoms improved. On the 17th a light bandage was applied round the abdomen. A colocynth pill procured two dark fetid stools intermixed with pus. In three weeks the discharge had completely ceased, and my patient, who was able to walk about, was rapidly regaining his wonted health. At the present time, April 1848, he continues well.

Part Second.

REVIEWS.

Principles of Medicine, comprising General Pathology and Therapeutics, and a Brief General View of Etiology, Nosology, Semiology, Diagnosis, Prognosis, and Hygienics. By CHARLES J. B. WILLIAMS, M.D., F.R.S., &c. Second Edition, considerably enlarged. London: 1848. 8vo. Pp. 533.

THE actual state of pathology must be regarded as one of transition. Almost every school of medicine contains one or more enthusiastic labourers who are cultivating morbid anatomy on the basis of pathological histology. Almost every laboratory possesses men who are working out the problems connected with the chemistry of organic

tissues in a state of health and disease. At the present moment both pathological histology and organic chemistry are advancing with astonishing rapidity, and our own pages offer a convincing proof that scarcely a month passes without new facts being discovered and new ideas evolved of the utmost scientific importance. So quickly indeed does one new fact succeed another, that the time necessary for carrying a large work through the press is almost sufficient to vitiate its doctrines. Hence, all systematic books on pathology are necessarily premature. They are like infants born before their time; though newly formed they bear the characteristic features of old age, and rapidly sink for want of vital strength.

Five years have elapsed since the appearance of the first edition of Dr Williams' work on the principles of medicine. A copy of the second edition, now before us, is nearly twice the bulk of its predecessor. This is owing to the accumulation and addition of new facts and observations. We nowhere find, however, any important change in doctrine. On the contrary, the author tells us in his preface, "that in very few instances has it been necessary to retract or supersede the inferences and views set forth in the first edition." Knowing, as we do, how difficult a task it is to eradicate inferences and views once adopted, we are not surprised at this, although we are prepared to maintain, that very few of those regarded with so much favour by Dr Williams are capable of standing the test of time, if, indeed, they may not already be regarded as obsolete. It is, of course, impossible for us to criticise one tenth part of the doubtful and undecided questions treated of in this book; we shall, therefore, limit ourselves to the consideration of the author's opinions on subjects stated in the preface as having "been confirmed and extended by recent researches to a degree that has not less surprised than convinced him of their truth." These subjects are *Congestion, Dropsy, Determination of Blood, Inflammation, and Deposits.*

CONGESTION.

The older pathologists considered congestion to be connected either with an asthenic or sthenic state of the system, and to depend upon determination of blood on the one hand, or atony of the vessels on the other. These views are adopted by our author, who further speaks of these two kinds of congestion as being attended, the first with diminished, and the second with increased motion.

The chief causes of congestion with diminished motion are, according to Dr Williams, first, those of venous obstruction; second, those of atony of the vessels. Instances of the first from ligature or other obstructing causes are so common and self-evident, that we need not dwell upon them. Congestion from atony of the vessels may depend on numerous circumstances. It may be general, as in cases of debility, adynamic fevers, and the sinking which precedes death; or it may be local, from long continuance of a part in one position, from cold, the influence of malaria, and over excitement. Under the last head, we, for the first time, meet (p. 183) with an

allusion to a theory to which the author attaches great importance, not only in the production of congestion, but of inflammation also. We allude to the formation of colourless corpuscles in the vessels, which, combined with diminished tone of the vascular walls, causes them to be obstructed. This theory we object to *in toto*; but as we shall allude to it again under the head of inflammation, we need not enter upon its consideration at present. We need only cite a passage from Mr Erichsen's paper on Asphyxia, quoted by the author at length, who says, "Nor have I *ever* been able to discover any obstructions in the vessels in consequence of the adhesion of colourless globules to the sides,—a phenomenon that I especially watched for, and which has by several been supposed to occur."

Vessels which have lost their tone, according to the author, become inelastic and tortuous, and the very accumulation of blood in them opposes an increasing obstacle to its passage through them. This point he illustrates by some experiments in which water was forced from a syringe through brass tubes and coils of intestine, the latter being twice as long and twice the diameter of the former. The result was, that the small metal tube discharged from two to five times the quantity discharged by the larger but membranous tube. These experiments illustrate a principle that is too little considered in animal and general physics; *the loss or neutralization of force by misdirection*. In health the blood-vessels are so arranged as to make the most of the heart's propulsive power, and transfer it throughout their whole length; but when dilated, tortuous, flaccid, and otherwise altered, they misdirect and exhaust it; a sufficiency does not remain for the onward propulsion of the blood, which, therefore, stagnates and accumulates in the congested vessels.

DROPSY.

Congestion leads to an increased transudation from the distended capillaries, causing effusions of the watery and saline part of the blood, more or less impregnated with albumen, and sometimes even with fibrin, as exemplified in the fluids of fluxes and dropsies. Thus, congestion of the bronchi causes bronchorrhœa; congestion of the intestines causes diarrhœa; congestion of the uterus, leucorrhœa; congestion of the kidneys, watery and sometimes albuminous urine; congestion of the lungs and pleura, hydrothorax; of the heart, hydropericardium; of the abdomen, ascites, &c. Dr Williams considers this result to be purely physical, and the different characters of the effusion to depend not only on the amount of distension of the vessels, but on the condition of the blood. A watery state of this fluid promotes transudation, whilst a highly albuminous and fibrinous blood requires more pressure to make its watery parts pass through the coats of the congested vessels. In the latter case the fluid often contains self-coagulating fibrin, the "fibrinous dropsies" of Vogel. To this kind of dropsy the author refers the gelatinous masses of lymph often found in the peritoneal sac of the abdomen, the fluids of bronchorrhœa, mucous diarrhœa, and leucorrhœa, the

pseudo-membranous fibres occasionally effused on mucous surfaces, and albuminous urine. He considers also that hypertrophy may result from this form of congestion.

We are of opinion that Dr Williams has here confounded together two kinds of congestion, and that the phenomena he has latterly described, the fibrinous dropsies of Vogel, are erroneously ascribed by him to congestion arising either from obstruction or atony of the vessels. We will undertake to say, that there is no undoubted instance of a mere mechanical obstruction, either by ligature or otherwise on a vein, that has ever caused a true exudation. The author, indeed, refers to the experiments of Dr Robinson of Newcastle; but when we consider the injury to which the kidneys, in his experiments, were exposed, we can have no difficulty in attributing the effects to an inflammation rather than to a mere congestion. In short, we believe that two distinct lesions have been confounded together by Dr Williams,—that the pressure theory with respect to veins will not hold; and that, if it be applied to the capillaries, it ought to be considered as a result of inflammation, and treated of under the author's next section, namely, congestion with motion increased, or what, following the older pathologists, he denominates

DETERMINATION OF BLOOD.

In discussing this subject, Dr Williams first refers to examples of local active hyperæmia in health, such as the face and neck in blushing, the uterus and breasts at the period of gestation and lactation, the gums during the period of dentition, and the antlers of the stag at the season of their development. In disease, he alludes to cases of what is commonly called "determination of blood" to the head, which, it is stated, have been seen to come on first with heating of the carotids, then flushing of the face and head, suffusion of the eyes, and sensations of distraction in the head.

With all deference to the author, we cannot help suspecting some fallacy in this observation. To suppose that congestion of the brain, and, as it is said, fits of epilepsy and hysteria, are preceded by throbbing of the arteries; or, in other words, that this throbbing of the carotids is the first in the sequence of changes leading to the congestion, is opposed to all known facts on the subject. The carotids have no such power of active contraction, independent of the heart on the one hand, or the capillaries on the other. No doubt, the arteries leading to inflamed or actively congested parts, do throb in an unnatural manner; but we believe that this is a result, and not a cause. In no place is this throbbing so well observed or felt as in the finger from a festered wound, yet this is evidently a propagation backwards from the injured part. It is the result of stimuli originally applied to the extreme vessels.

The author next alludes to the action of local stimuli to the vessels, such as snuff to the nose and eyes, spices to the mouth, and food to the stomach, which, he says, produce local determinations of blood. Here we cannot recognise any "determination." We ob-

serve only an impression on the contractility of the vessels, the effect of which is to cause enlargement of them, and as a matter of course congestion or hyperæmia. Nothing more. But what does the author mean by this term determination? He tells us, that, on irritating a frog's web, we see the arteries become enlarged, supplying a larger and more impulsive flow of blood to the capillaries and veins, which all become enlarged also; and the whole vascular plexus, including vessels which before scarcely admitted red particles, then become the channels of a much increased current. "This is determination of blood."

But here again we must differ from our author on a matter of fact. We have performed the experiment frequently, but could never observe that the arteries were the vessels first affected. On the contrary, it always appeared to us, that the influence of the stimulus spread over a portion of the surface at once, that it was impossible to distinguish any difference between the effect on the capillaries and the effect on the minute arteries and veins, but that in all cases the smaller vessels (the capillaries and minute arteries) were influenced first, and the larger branches last. We hold, therefore, that the idea of "determination of blood" existing in the body is altogether erroneous, and that in no case do the arterial trunks take upon themselves the initiative in pouring blood into the minute vessels.

Dr Williams next enters into an argument to prove, that his determination of blood depends upon physical causes, upon purely hydraulic laws; but differing from him, as we do, with respect to the facts on which he rests his theory, it is unnecessary for us to attempt its refutation. The principal error, we think, lies in attributing too much to physical, and too little to vital causes, and in almost entirely overlooking that wonderfully great contractile and vital power of the capillaries, first demonstrated physiologically by John Hunter, and shown to depend upon a peculiar structure by Henle.

INFLAMMATION.

Dr Williams defines inflammation to be "too much blood in a part, with motion (of that blood) partly increased, partly diminished." The increased motion is, according to him, dependent on "determination," a subject we need only again allude to as a proof of the inconsistency of the whole theory. The "determination," that is, the throbbing of the arteries, can never exist in an inflamed part. It is in the neighbourhood only, where the vessel is still pervious, that the phenomenon is manifested, and we have endeavoured to show that this is the effect and not the cause. All the facts with which we are acquainted prove that the condition of the vessels actually causing inflammation is one of obstruction, with complete stoppage of blood, and it is the cause of this stoppage which constitutes the only difficult subject of inquiry connected with the subject.

Here, again, we find that the author's opinions regarding a tonic congestion and "determination," are made the basis of a complex view

of inflammation, in which, of course, the contractility of the capillaries is denied, and the influence of hydraulic laws altogether depended on. Even these powerful aids, however, do not enable him to explain the stagnation of the blood in inflammation, and he has consequently recourse to another theory, said, like those which have preceded, to be founded on well observed facts, but like them, as we shall endeavour to show, unable to bear the test of close examination. This theory provides for another mechanical cause of obstruction, originating in the blood, by the formation of an increased number of colourless corpuscles, which stick to the sides of the vessels, and block them up. This would certainly be a very satisfactory cause did it really exist; but in this, once more, the author's facts have been wrongly interpreted, and made subservient to an hypothesis which has no real foundation.

Is it then a fact, as Dr Williams and Mr Addison assert, that a large number of colourless corpuscles are formed in the vessels of the frog's web, on irritating it? Dr Hughes Bennett, in a paper published in this *Journal* for January 1847, says, No. We quote the passage in that paper which refers to this subject, so that our readers may judge for themselves:—

"In the autumn of 1843," says Dr Bennett, "we carefully repeated all the experiments described by Mr Addison and Dr Williams, with Dr Redfern, now professor of anatomy in King's College, Aberdeen. Since then we have repeated them four separate times, twice publicly in November before the class of pathology and practice of physic, and twice in June, in conjunction with the gentlemen attending the class on histology. We have just terminated a series of these experiments for the fifth time, and the results have been so decided and satisfactory, as to render it imperative upon us to publish them, and to demonstrate the errors into which the promulgators of this doctrine have fallen.

"It is true that in young frogs, and in many full grown ones fresh from the country, the colourless corpuscles are very numerous. It is equally true that in old frogs, and in many which have been long kept, they are few in number. It is in the web of such animals that, according to Dr Williams, the increase of the colourless corpuscles, as the results of irritation, are well observed. Now, we have applied salt, capsicum, and essential oils, and have never seen these corpuscles increased in number, relatively to the yellow corpuscles. When, by means of these irritants in young frogs, congestion is occasioned, all the blood globules within the vessel increase in number, and that of the colourless corpuscles is of course augmented. But the yellow corpuscles are also more numerous, and neither we nor any of the assistants (and more especially Dr Wilkinson, our assistant of this year), have ever seen that the former existed relatively in greater proportion.

"Again, in old frogs, in the vessels of which the colourless corpuscles are very few, we have frequently excited stoppage and exudation, without seeing any colourless corpuscles from the commencement of the process to the end. In other words, on the addition of acetic acid, or other irritants, all the phenomena we have described make their appearance, producing oscillation and stoppage of the current of blood, crowding together of the yellow corpuscles, without the appearance of one colourless one. It often results, however, from the action of re-agents, especially of acetic acid, that the nuclei of the epidermic cells covering the web are rendered very distinct. These nuclei are round colourless bodies, exactly resembling in size and appearance the colourless blood corpuscles. It consequently happens, that the vessels running between the two layers of epidermis present at different foci an appearance as if the

internal wall of the vessel was studded with colourless corpuscles. This, I am persuaded, is the phenomenon which has given rise to the idea of the calibre of the vessel being crowded with them. Under such circumstances, however, it may be seen, that the parenchyma of the web in the meshes of the capillary plexus is also studded over with similar nuclei, owing, of course, to the epidermis being spread uniformly over the whole web. Mr Addison says, that colourless corpuscles escape from the vessel into the surrounding tissue, a circumstance which is physically impossible without rupture of the vascular wall, although the origin of the statement may probably be explained by the facts we have pointed out.

"If further proof is required, it may be found on the examination of the circulation in the tail of the tadpole. Here the vessels are so easily excited and paralysed by irritants, that before stoppage of the blood they frequently burst, causing extravasation of blood, and yet without any increase in the number of colourless corpuscles. Moreover, not only have none of the histologists of Germany, who have carefully watched the inflammatory process, ever observed an increase in the number of these bodies; but this supposed occurrence is directly negatived by recent observations and experiments of Remak, who has shown that their increased number in the blood follows large abstractions of that fluid, and that the portions first drawn in inflammations contain very few. It may be concluded, then, that there is no increase of the white corpuscles in inflammation; no crowding together of them, so as to produce obstruction of the vessel; no escape consequently into the surrounding tissue; and that the observers who have stated these circumstances to have occurred, have mistaken the nuclei of the epidermic cells in the web of the frog's foot for these structures."—*Monthly Journal*, January, pp. 603-605.

The following is Dr Williams' commentary on the above passage—

"The presence of the pale corpuscles in inflamed vessels has been questioned by Dr Hughes Bennett, who hints that Mr Addison and myself may have mistaken for them epithelium cells lining the vessels; but the plain description above given, and which I have repeatedly verified, admits of no such interpretation. The granular corpuscles may be distinctly seen to roll sluggishly, and with a *dragtail* before they stop; which they obviously do by adhesion to the interior of the vessel. I cannot understand why Dr Bennett has failed to see so clear an appearance."—P. 266.

Now, a perusal of Dr Bennett's statements will prove that he *has* seen all the appearances described by Dr Williams. The facts themselves are correct as far as they go. What is denied is, that such appearances are the invariable forerunners of obstruction in inflamed vessels, and are the cause of the stoppage in the circulation. What Dr Bennett asserts is, that he and his assistants have again and again watched the whole process of inflammatory obstructions in old frogs, without seeing any of those white corpuscles; and, on the other hand, they have seen all the phenomena described by Dr Williams in young frogs, without inflammation or obstruction having occurred. The inference is, that Dr Williams has mistaken an accidental phenomenon for an essential part of the process, and attributed a uniform effect to an occasional occurrence. On the other hand, the only explanation offered by Dr Bennett of Dr Williams' error, is the supposition that he had mistaken the nuclei of the epidermic cells running above and below the vessel (not the epithelium cells lining the vessels, as erroneously stated by Dr W.),

which often, from the action of acetic acid, give to the vessels the appearance of being crowded with colourless corpuscles.

Our readers must judge for themselves as to the correctness of these opposing statements. Inexperienced persons, however, should not be misled by the diagram given by Dr Williams, p. 262, which he says "exhibits the appearance of a small portion of the capillaries." From the size of the vessels in relation to the blood globules they are probably small veins. For ourselves, we are by no means inclined to believe that the mechanical notions of Dr Williams, and the creation of a number of colourless corpuscles to stop up the vessels, can explain the nature of inflammation. But not to dwell upon the experiments of Remak, which appear to be unknown to the author, there is a series of cases on record, quite sufficient in itself to overthrow the theory. We allude to the four cases by Craigie, Hughes Bennett,¹ Virchow,² and Fuller,³ in which the blood was crowded with colourless corpuscles in every vessel, without any of the phenomena of inflammation being present, nor any of its usual appearances existing in any of the solid tissues. With these cases also, the author seems to be entirely unacquainted.

We hold, therefore, that this peculiar theory of Dr Williams' is not only not proven, but is extremely improbable; and think moreover, that the physical and mechanical causes to which the author ascribes the phenomena, are altogether inefficient for the purpose. Whether the views of the vitalists, and more especially the vital attraction and repulsion theory of Professor Alison, be better founded, we shall not stop to enquire. Certainly it is not free from objection: but on the whole we have more faith in it than in the mechanical notions of our author.

We might dwell at considerable length on the author's account of the results of inflammation, in which the same imperfections in appreciating facts, and the same unacquaintance with the labours of modern pathologists, are every where observable, as in the points we have just noticed. But, confining ourselves to those matters, which, according to Dr Williams, have been most perfected by recent researches, we have next to notice what is said on

DEPOSITS.

The subject of deposits in or upon textures, constitutes section 6 of chapter iv. on structural diseases, or diseases of nutrition. Here we cannot refrain from observing, that nothing can exhibit so clearly the faulty arrangement of a work on pathology, and the confused notions of its author, as separating the phenomena of inflammation from diseases of nutrition. Surely that process which perverts the growth of a part, leads to formations of pus, lymph, ulceration, and gangrene, must, more than any other, be regarded as

¹ Edin. Med. and Surg. Journal, October 1845.

² *Frorieps*, N. Notizen, No. 780.

³ *Lancet*, July 1846.

a lesion of nutrition. Yet Dr Williams makes inflammation and its phenomena constitute one section, and diseases of nutrition another. It is under this latter head the author describes hypertrophy, atrophy, induration, softening, transformation, and degeneration of textures, deposits, non-malignant growths, malignant growths, and disorders of mechanism. On each of these subjects we have much to find fault with; indeed none of them may be considered as communicating to us the actual state of science regarding them. What is said as to deposits will serve to illustrate this statement.

The term *deposits* is applied by the author "to matters which result from an overflow of the nutritive material beyond what is necessary to nourish the textures themselves." These matters he has divided into *euplastic*, *cacoplastic*, and *aplastic*, by which barbarous words he means substances that exhibit a perfect, imperfect, or absent organization. We are surprised to see that the euplastic deposits are formed of cicatrices, which in our opinion ought, according to the author's views, to be considered as cacoplastic. We find, however, that Dr Williams does not mean cicatrices properly so called, but regenerations of perfect tissue.

For an account of the healing of wounds the author is indebted to Carpenter's physiology, and it is to be regretted that he should have had recourse to a mere compilation, when so many masterly accounts of the process, founded on original observation, are to be obtained in the works of British and continental pathologists. But in this, as well as throughout the work, there is to be discovered a very deficient knowledge of the labours of his contemporaries, and especially of the investigators in Germany.

Under the head of cacoplastic and aplastic deposits, there is a description of true cicatrices, which we are told "exhibit various degrees of organization, some being vascular, and some not." Here, as in many other places in the work, the author confounds organization and vascularity with each other, an error that no one acquainted with the present state of physiological-pathology could have committed.

The structure of tubercle is described by the author as consisting of a "predominance of minute and often irregular granules, and the comparative absence of fibres and cells, of which mere traces are seen, at least in the older specimens. The granules are aggregated together by an amorphous material, the solidity of which gives hardness and some translucency to the mass; for acetic acid or alkalies, which dissolve this cement, loosen the granules and render them distinct. The chemical nature of granular tubercle is albuminous, with some gelatine and a little fat, the latter in very minute proportion, and occupying the centre of some granules; and the gelatine being probably the amorphous cement just noticed," p. 391. Now, these so called "granules" are, we presume, the tubercle corpuscles of modern pathologists. They cannot be granules properly so called; for the author tells us, p. 269, that a

granule appears "as a light spot, surrounded by a dark circle." Calling, then, these bodies granules, is not only contradictory, but, in the case of a structure having contents, is faulty in the extreme. Why has not the author given us the admirable description of these corpuscles by Lebert, instead of an account so vague and confused, that even few histologists can understand what is meant, not to speak of the generality of his readers? But here, again, no allusion is made to the accurate researches of Lebert, with whose labours the author seems to be wholly unacquainted. As to the amorphous matter which is associated with tubercle being gelatine, we know of no fact which can support such a statement. Most assuredly the author has not given us one. Neither has he noticed the numerous analyses of tubercle by Glover of Newcastle.

There are numerous speculations in this section with which we cannot agree, and which a more extended knowledge of the writings of other pathologists would have prevented the author from forming, especially the idea that tubercular matter may form within the vessels, p. 398; that tubercle is most common in the lungs, because they are the chief seat of the formation of fibrin, and so on.

Dr Williams tells us, that cod liver oil is assuredly the most efficacious of all medicinal agents in the treatment of cacoplastic and aplastic deposits, and is one which, after two years' constant experience in its use, is still frequently surprising him by the wonders that it occasionally works even in aggravated and advanced cases of scrofula, mesenteric disease, pulmonary consumption, chronic pneumonia and pleurisy, and chronic rheumatism. He conceives it to act as fatty matter. The nuclei or rudimental molecules of all structures appear to consist of fat, which the oil in its highly divisible state supplies and renews in the manner most conducive to active and healthy nutrition. We are surprised to find that this explanation of the action of the oil is put forth by Dr Williams as his own, and alluded to both in the preface and in the text (p. 404) as the result of the author's reflections on the subject. Though unacquainted with modern researches made so far off as France and Germany, he might surely have known that this very theory had been formerly published by Dr Hughes Bennett in 1841, and publicly taught by him at no further distance than Edinburgh, for the last seven years.

Our limits prevent us from proceeding further. We have confined ourselves to a notice of those portions of the work considered by the author himself as being most perfect; and we need not say that if such be the correctness of those inferences and views, which in the opinion of Dr Williams have been confirmed and extended by recent researches, the other portions we have left unnoticed are open to even more objections. And such is the fact. Now, as stated at the commencement of the article, this is not altogether owing to Dr Williams, but to the imperfect state of our knowledge. The error consists in speaking too positively on subjects which are

still doubtful, and in writing a systematic book on what cannot be systematized. We had reason to expect, however, a candid and fair statement of the opinions of others; but we regret to say, that either owing to inadvertence, or to a want of information, this has not been furnished. Had the author perused the works and memoirs of Remak, Gruby, Rokitsansky, Engel, Henle, Lebert, Virchow, Bruch, and other members of the physiologico-pathological school of Germany, it would doubtless have merited a different notice from us.

Notwithstanding this work contains so much that is deficient in observation, research, and correct theoretical deduction, we have no hesitation in saying, that it also possesses merits of a very high order, which will not only render it acceptable to the medical practitioners of this country, but probably make it valuable to our continental brethren. In Germany, indeed, it is likely to be even more useful than in England; for whilst its errors and deficiencies will be palpable to the foreign pathologist, the chances are, that if these do not induce him to throw the work down too soon, he will obtain from it a degree of information that none of the books published in his own country will afford him. We allude to that portion of Dr Williams' "Principles" which refers to treatment. In all that is said on this subject, we recognise the enlightened and practical physician. No doubt we owe to Germany much of what is known of pathology as a science. There, it is pushed forward with a rapidity far too great for a London practitioner. But it is destitute of that great aim which can alone make it valuable, namely, the cure of disease. In uniting theory with practice, and in thus following the great examples of the Hunters and the Bells, Dr Williams has done well. Such, we trust, will always be the chief characteristic of British works on medical science. Dr Williams has many claims to the gratitude of the cultivators of medicine; but the work we have just noticed, is sufficient to prove that these claims do not so much rest upon his knowledge of pathology, as on his powers of diagnosis and skill as a practical physician.

Traité de l'Art de Formuler, ou Notions de Pharmacologie appliquée à la Médecine. Par M. MIALHE, M.D., Pharmacien, Professeur agrégé à la Faculté de Médecine. Paris: 1845. 8vo. Pp. 518.

A Treatise on the Art of Prescribing. By M. MIALHE, M.D., &c. Paris: 1845.

THE ordinary run of treatises on the subject announced in the title of this work are, for the most part, characterised by the absence of original matter; the book before us is distinguished by the opposite quality. In Mialhe's work we are furnished with an extensive series of original experiments, having for their object, on the one hand, the

explanation of the action of medicinal substances, and, on the other, the improvement of the formulæ made use of in their administration. The "formules rationnelles" of this writer are founded on a careful consideration of the chemical reactions that medicines undergo in the stomach and intestines, and are devised with the view of securing, as far as possible, the absorption of the whole dose of the remedy prescribed, in every instance where its entrance into the blood is essential to its therapeutic action.

Five chapters are occupied with general considerations on the absorption of medicines. As the fundamental proposition of his treatise, our author contends, that "every substance capable of exerting a *remote* action on the animal economy is soluble, or susceptible of becoming so in the fluids of the body" * * * "such substances alone being capable of absorption,"—pp. 17 and 22; and his researches have served to solve several apparent exceptions to this law. His experiments show that sulphur, phosphorus, metallic arsenic, the insoluble compounds of lead, antimony, mercury, silver, &c., the action of which is often cited to prove an opposite opinion, are all chemically influenced, and rendered wholly, or in part, soluble by the agency of the gastro-intestinal fluids; and to the change thus effected, we can have little hesitation in attributing their physiological action.

If, however, the recent observations of Oesterlen are to be relied upon, solubility does not appear so exclusively essential to absorption. This writer has observed that finely powdered charcoal, when administered by the mouth, finds its way into the blood, in which fluid the carbonaceous particles may be detected by the microscope. Admitting Oesterlen's observation to be correct, it is improbable that charcoal or any analogous substance could, in this way, be introduced into the blood in sufficient quantity to act upon the economy.

In relation to absorption, Mialhe divides the articles of the *materia medica* into two classes. The *first* class comprehends those substances which are soluble, and hence capable of immediate absorption;—it includes potassa, soda, ammonia; all the soluble vegetable acids; some mineral acids; arsenious and arsenic acids; all alkaline salts; and of the metallic salts, all those which do not coagulate albumen, as the cyanides of potassium, iron, mercury, &c.; some vegetable alkaloids, and indeed we may say all, as there are none entirely insoluble in water.

The *second* class comprises all insoluble drugs which require the intervention of the gastro-intestinal fluids to effect their absorption, and is divided into—A, substances which are rendered soluble by the *acids of the gastric juice*. This division includes all the metals except silver, gold, &c., which have no action on the economy when taken in the metallic state, all the insoluble metallic oxides, and the sparingly soluble vegetable alkaloids. B, substances which are rendered soluble by the *alkalis of the intestinal canal*. Sulphur, phosphorus, iodine, almost all the mineral acids, tannic acid, and all the

insoluble organic acids; the electro-negative metallic oxides of antimony, tin, zinc; resins, fixed oils, &c., belong to this division. C, substances requiring for their absorption the intervention of *alkaline chlorides*. This division includes all the oxides and salts of lead, mercury, silver, gold, and platinum. Their absorption may be effected throughout the entire length of the alimentary canal, or wherever alkaline chlorides are present in the secretions.

That a classification of this kind has considerable practical value is at once evident. In administering a drug belonging to division A, very little fluid should be given along with it, as a large quantity not only weakens the solvent power of the gastric juice, but also hastens the passage of the drug into the intestines, where it meets an alkaline reaction. Of course, alkaline drinks should be here avoided; while it is equally evident, that to obtain the maximum effect of a medicine included in division B, acid drinks should not be taken.

But as the quantity of acids, alkalis, and of salts contained in the fluids of the body, is limited, it follows that the action of insoluble bodies is also limited, and often bears no relation to the amount of medicine taken. We are thus enabled to understand why sixty grains of calomel should not act with twelve times the energy of five grains of the same medicine, or why so little difference should be observed in the remote effects of small and large doses of such drugs as the carbonate of the peroxide of iron, nitrate of bismuth, oxide of silver, &c.

In the use of soluble medicines which are not decomposed by the gastro-intestinal fluids, we can predict with considerable accuracy the effect which a given dose will produce, and can increase or diminish that effect by increasing or diminishing the dose. But in the case of insoluble drugs, many circumstances may interfere to prevent the chemical reaction necessary to render them active; and consequently we find that the same dose may in different individuals, and at different times, produce very unequal results. Calomel, oxide of antimony, and other insoluble medicines, afford constant illustrations of this remark. In many diseases, the secretions undergo important changes in their chemical composition, and in their power of acting on insoluble bodies, which present in consequence unusual inertness or activity, as the case may be. Many anomalies of this kind have been hitherto referred to idiosyncrasy. It is further clear, that, beyond a certain limit determined by the solvent power of the gastric and intestinal fluids, we cannot increase the remote action of an insoluble drug by augmenting the dose. When this limit is exceeded, a portion of the medicine remains undissolved, and is either expelled with the fæces; or what is very likely to happen, at least if the use of the drug be continued for some time, it accumulates in the stomach and intestines, and afterwards leads to unpleasant consequences. On this subject our author makes the following useful remarks:—

"When," he observes, "an insoluble medicinal compound is introduced into the system, and cannot be entirely dissolved by the gastric and intestinal fluids, the insoluble portion of this body, or the insoluble whole, either traverses the entire length of the alimentary tube, to be expelled with the feces, or is arrested in its course, and lodges for a time in some of the folds of the intestinal mucous membrane. Thus, the inconsiderate employment of caustic magnesia has given rise in the stomach of the gouty to extraordinary magnesian incrustation; insoluble preparations of iron, and especially the subcarbonate of the peroxide, administered in too large doses, often occasion intestinal concretions. The accumulations may even occasionally be the direct consequence of the exhibition of too great a quantity of the medicine. But the insoluble matters thus accumulated do not all present the same dangers; those on which the vital fluids exert no effect, act on the surfaces with which they are in contact only as foreign bodies—that is, by causing irritation and symptoms of inflammation; while matters capable of becoming soluble in consequence of a change in the quantity, or in the composition of the visceral humours, may thus become active, or often poisonous, and by their absorption give rise to severe or even fatal effects.

"It is thus that calomel, given as a purgative in large doses, occasionally causes ptyalism, and deeply affects the system; so also basic or officinal sulphate of quinine, administered in the dose of several grammes daily, though at first producing no remarkable physiological effect, has all at once given rise to symptoms of poisoning, followed by death. A few glasses of tartaric lemonade have occasioned vomiting and diarrhoea in a patient who had taken protoxide of antimony some days before, it is scarcely necessary to add from the formation of tartarized antimony. Ioduretted water, given to a patient affected with a cutaneous disease, who had a short time before been taking calomel as an alterative, has given rise to most profuse salivation, occasioned by the biniodide of mercury, resulting from the action of the ioduretted water on protochloride of mercury still remaining in the system."—P. 260, *et seq.*

An important practical conclusion to be derived from the foregoing is, that insoluble agents should not be exhibited in single large doses, but in frequently repeated small doses, so as to expose the medicine to the solvent power of a large quantity of the fluids of the body. In practice this precept is frequently followed; but its *rationale* is, we believe, not generally understood.

The proportion of water taken into the stomach along with an insoluble medicine, exerts considerable influence on its therapeutic action; the solvent power of the secretions being diminished by large dilution. Theoretically, this is exactly what we should expect; but the following experiments will enable the reader to see clearly the full import of the proposition that we have just laid down. Under the influence of alkaline chlorides in solution, calomel undergoes partial conversion into corrosive sublimate and metallic mercury, to which change, as will be afterwards seen in the course of our remarks, we have good reason to attribute the physiological action of this important remedy. In four experiments calomel, six decigr, common salt and muriate of ammonia, of each six decigr, and a different proportion of distilled water, were placed in contact during twenty-four hours, at a temperature of from 40° to 50° centig. The quantity of distilled water used, and the amount of corrosive sublimate formed, in each experiment, were as follows:—

Distilled water 5 grammes.	Corros. subl. produced 24 milliga.
" " 10 grammes.	" " " 19 milliga.
" " 20 grammes.	" " " 12 milliga.
" " 40 grammes.	" " " 9 milliga.

Clearly showing, that the quantity of corrosive sublimate produced is greatest when the alkaline solution is most concentrated.

Of the sixth chapter, two sections are devoted to the consideration of the changes which medicines are supposed to suffer in the blood, and the mode in which they act on the economy. They contain much ingenious speculation, and merit careful perusal.

The greater portion of the work contains a detailed examination of the individual members of the *materia medica*. We have space to notice only a few of the more important.

Sulphur.—In this drug we have an excellent illustration of an insoluble body capable of exerting a remote or constitutional action on the economy, and the question arises, How is its absorption effected? Mialhe's experiments show that, when exposed to the action of alkaline carbonates in solution, sulphur is transformed in part into the sulphuret and hypo-sulphite of the alkali, compounds which are soluble, and hence capable of absorption. It is therefore probable, that sulphur enters the circulation only in virtue of the changes which it undergoes by contact with the alkalis of the intestinal fluids; in favour of which view, it may be urged that the most active preparations of sulphur are the alkaline sulphurets, or those which contain them. Its action is much promoted by the addition of an equal part of carbonate of magnesia, which probably serves to saturate the free acids of the stomach, and so enable the alkalis and their carbonates to operate more effectually.

Mialhe advances, as another argument in favour of this view, the fact, that the energy of sulphur ointment as an anti-psoric remedy is much enhanced by the addition of the carbonate of potassa; but the latter remedy alone forms perhaps the most active application in scabies that we possess.

Iron.—In France it is a common opinion, that in the treatment of chlorosis and other affections demanding the tonic powers of iron, those ferruginous preparations only are efficacious which contain the protoxide, or the metal itself, which in the stomach is converted into protoxide; further, that the protoxide must be united with carbonic acid or some organic acid that can be assimilated, as the citric or lactic; and lastly, that all the salts of the peroxide of iron, and all ferruginous combinations with the strong mineral acids—as the sulphuric, phosphoric, or muriatic—are not assimilated, and are only useful as astringents (Bouchardat, Gélis, and Conté, *Annuaire de Thérapeutique*). Mialhe maintains, and very justly, that clinical observation is altogether opposed to these conclusions, as is shown in the recognised value of such preparations as the subcarbonate and citrate of the peroxide, the hydrated peroxide, and the permuriate

of iron. On his part, he advances the following propositions:—

1. "That all ferruginous preparations, soluble or capable of solution in the acids of the gastric juice, and susceptible of decomposition by the alkalis or their carbonates contained in the blood, may be advantageously employed in the treatment of chlorosis." This includes all the preparations of iron in common use; as the pure metal, its combinations with oxygen, the chloride, bromide, iodide, and, without exception, all the oxisalts of iron. All these suffer decomposition in the blood, the alkalis of which unite with the acid, and the base is set at liberty. This is the property that, according to Mialhe, constitutes the essential condition of the tonic (or, to speak more precisely, "reconstituant"¹) powers of a ferruginous preparation. As the theory indicates, salts of this class are slow to appear in the urine, and if given in small quantity, cannot, with one or two exceptions, be detected in that fluid.

2. "That none of the martial compounds, soluble or capable of solution in the acids of the stomach, but not susceptible of decomposition by the alkalis of the blood, can be of any service in the treatment of chlorosis." This class comprises only a few compounds; as the ferro-cyanide of potassium, the sulpho-cyanide of potassium and iron, &c. These traverse the circulation unchanged, and soon appear in the urine.

We know, that in chlorosis the constituent parts of the blood which are deficient are the iron and the blood globules, and that these are promptly restored by a course of chalybeates. Whether, in producing this effect, the iron acts simply as a general tonic, or whether it is assimilated, and enters directly into the formation of the blood globule, the present state of our knowledge does not enable us certainly to determine. The latter hypothesis is, we think, very probable; but Mialhe, without giving any satisfactory reason, assumes it as an established fact. The arguments in favour of it are these, (1.) That the other general tonics have not the same effect as iron in chlorosis; (2.) That those chalybeates which, from their nature, are detained in the blood, are the most efficacious.

Mialhe goes on to consider the condition in which iron enters into the formation of the globule. Liebig had already shown that the iron contained in the blood was combined with oxygen, and Mialhe observes, that the probabilities are all in favour of the peroxide being the active agent; it is unalterable in the air, and its salts are in general stable, and present a red colour more or less resembling blood. If iron were assimilated only in the first degree of oxidation, it is very unlikely that the persalts would form such energetic preparations. He believes that the protoxide only becomes active by passing to the higher degree of oxidation. His ingenuity carries him a little further, and offers us the following hypothesis

¹ Trousseau and Mialhe mean to indicate by the use of this term, that the iron is retained in the blood and assimilated.

respecting the formation of the blood globule. The ferruginous salt and the albuminate of soda contained in the blood suffer mutual decomposition; there are formed a new salt of soda and the albuminate of iron, which, according to our author, constitutes the base of the blood globule. An experiment, advanced in support of this opinion, merits attention. When to a solution of albumen we add a perfectly neutral persalt of iron, no precipitate forms; but on adding to this mixture a certain quantity of chloride of sodium, a copious precipitate falls. Now, we know, that while the globules of the blood are soluble in distilled water, they are not so in a solution of common salt.

When an insoluble preparation of iron is used, it should be administered in frequent small doses, and its use continued for a considerable period, as it acquires activity by solution in the acids of the gastric juice, which exist only in small quantity. Among the insoluble preparations, he gives the first place to the metal in fine division and the carbonate of the protoxide, then come Ethiop's mineral and the hydrated peroxide. The insoluble protosalts are preferable to the insoluble persalts, on account of the greater facility with which they are acted on by the acids of the gastric juice.

The soluble preparations of iron are incomparably more active than the insoluble. Among these there are some, as the sulphate and muriate, the absorption of which is incomplete on account of their active astringent power. "Of the soluble compounds, those which have the least styptic taste are richest in iron, and are most readily absorbed, ought to be preferred; and, on these grounds, no ferruginous compound can be compared with the tartrate of potassa and the peroxide of iron."¹—(*To be concluded in next Number.*)

Remarks on the Employment of Anæsthetic Agents in Midwifery.

By G. T. GREAM, one of the Medical Officers of the Queen Charlotte's Lying-in Hospital; Fellow of the Royal Medical and Chirurgical Society, &c. &c. London: 1848.

IN December last, immediately after the anæsthetic effects of chloroform in midwifery were announced, a notice against it was published in the *Medical Times*, averring stoutly that anæsthesia could not be of so much advantage, as had been reported, in first deliveries, because in first deliveries the principal impediment "is," said the writer, "found in the pelvic bones and rigidity of joints." Of course, every tyro in obstetric knowledge was capable of detecting the intense absurdity of the allegation, that the *bones and joints* of the pelvis were the cause of delay in first labours. The author of this amusing averment was Mr Gream, a London "Late Lecturer on

Midwifery and the Diseases of Women and Children." He has stated his objections against anæsthesia in midwifery at a greater length in the present publication; and we have no doubt that, a century hence, Mr Gream's pamphlet will be prized and quoted as a literary medical curiosity in its way, and another proof of the absurd and lamentable lengths to which medical prejudices will sometimes lead medical men, whenever any thing new is proposed in practice.

We believe it is needless to encumber our pages with quotations from a work exhibiting such extraordinary unacquaintance with physiological and pathological science as this pamphlet betrays; but lest any of our readers, perchance, deem our verdict too harsh, we will give an extract at random. At p. 26, on the "latent effects" of ether and chloroform, Mr Gream illustrates these effects by the case of the young druggist at Aberdeen, Arthur Walker, who poisoned himself in February last with chloroform. The italics in the following extracts are, we beg to say, Mr Gream's own:—

"Dr Jamieson, who examined the body of Arthur Walker, in his report says: 'Lungs turgid, loaded with dark blood, and *extensively adherent to the walls*' of the chest, and the '*heart extensively adherent to the pericardium.*'¹ '*The walls of the right ventricle thin, cavity enlarged.*' That we find these appearances in individuals not unfrequently, it will be allowed; but the combination of them in a person habitually a drunkard, through the effects of etherization, at once gives rise to the suggestion that they have arisen from this cause."—(P. 26.)

These adhesions, Mr Gream proceeds to show, were probably "the effect primarily of *venous* congestion," produced by the chloroform inhalation; and he thinks "it is impossible to be certain," that those who have inhaled ether or chloroform "have not at *this* moment similar morbid adhesions."

We feel almost ashamed to offer a word of commentary upon such statements and reasoning as this. But surely no other man in the educated medical profession of the present day, except Mr Gream himself, could possibly believe that old and intimate adhesions between the opposed surfaces of the pleura or pericardium, such as were found in Walker's case at Aberdeen—adhesions evidently of very long standing—could have been produced by chloroform, the anæsthetic effects of which were only first discovered in Edinburgh a few weeks before Walker died.

¹ Mr Gream gives this and the other extracts from Dr Jamieson's report, both within inverted commas and in italics, thus doubly marking his wish that they should be regarded as actual and true quotations. Dr Jamieson's published report of the case (*Medical Gazette for February 25*) was the official one given in to the law authorities. Mr Gream has completely changed its meaning. The actual words of Dr Jamieson's report, regarding the state of the heart, are these, "The pericardium was generally and closely united to the heart by *old* adhesions, which had to be forcibly torn asunder with the finger," &c. Mr Gream has carefully omitted the word "old," as that word would have totally destroyed all the arguments he wished his "fair" readers to draw from the case. We need not say that this is a most unwarrantable and unjustifiable proceeding, and, we fear, must seriously compromise Mr Gream's character as a medical author.

If Mr Gream places any faith in his own reasoning (though, for the sake of Mr Gream's medical knowledge, we charitably hope he does not), he must further believe that Walker, for some few weeks before his death, was going about his usual occupations with acute or subacute Pleurisy and Pericarditis marching on in full career within him, and without producing a single symptom. Certainly Mr Gream must have uncommonly innocent and strange ideas of pathology and disease.—Patients operated on under anæsthesia die sometimes, like patients operated on without anæsthesia, though by no means so frequently. If Mr Gream will inquire at any of his surgical friends—or any one who knows pathology at the London hospitals—he will find that these surgical patients do not exhibit appearances of pleurisy and pericarditis more frequently, after ether or chloroform, than those who die without having inhaled ether or chloroform.

Mr Gream's other illustrations of the "real hazards" of ether and chloroform are all precisely of the same kind as the above, and all betray a similar want of temper and of knowledge. According to his catalogue of their "real hazards," almost every corporeal disease is the result of their inhalations, and a few mental also; for he humanely suggests to the "heartless mother," who seeks by anæsthetic means to avoid her own sufferings, whether this practice may not make an 'IDIOT' of her child; and that, no doubt, on grounds as good and just as Dr Rowley—another fashionable practitioner in London, and "Lecturer on the Practice of Physic"—gravely suggested half a century ago to the "heartless parents" of his day (when vaccination was first introduced), that an artificial cow-pox might certainly, in the human subject, ultimately lead to the growth of cow's hair upon the human skin, and cow's horns upon the human head. Nay, a coloured drawing of a case, in which he averred this last transformation to be taking place, was published by him as a frontispiece to a pamphlet against the horrors of vaccination, written in the exact spirit of Mr Gream's publication. We would suggest Mr Gream's own portrait as a fit frontispiece for his next edition. And perhaps some of his readers will recollect Byron's commentary on the poet, who, like Mr Gream, selected and sung for his hero of "An Idiot Boy:—

"A moonstruck, silly lad, who lost his way,
And, like his bard, so mingled night with day,
That all who viewed the Idiot in his glory,
Conceived the bard the Hero of the story."

There is one subject prominently brought forward in Mr Gream's pamphlet, intended, as its pages are, to "fall into the hands of persons not belonging to the medical profession," on which we would make one parting remark. He avers that the use of chloroform is calculated to excite improper sexual feelings and expressions in women, when given during labour. Some time ago, in consequence of seeing this gross mis-statement loathsomely repeated over and over again, by the same hand, in a London journal, we made extensive inquiries among

our obstetric friends in Edinburgh, who were constantly, and with unvarying success, using chloroform in midwifery, as to whether they had, in any case, observed any thing that could give rise to such an idea. Not one of them had ever remarked any thing of the kind, or any thing approaching to it. But to those who are themselves impure in thought, every thing, in others, looks and sounds impure. And, on the present point, we quite agree in the opinion which we heard expressed by Dr Simpson at the Medico-Chirurgical Society a few nights ago. Persons, he stated, sometimes, when imperfectly under chloroform, carry on for a time the same train of thoughts, and even of conversation, as they were engaged with when the inhalation was begun; and if some obstetricians find their patients talking obscenely when chloroformed, the fault does not depend upon the chloroform, but upon the coarse and improper previous conversation of themselves or their assistants.

After what we have stated it is almost unnecessary to add, that Mr Gream makes very unscrupulous and distorted misquotations throughout his pamphlet, and revels very largely in a number of very frightful but very apocryphal cases and facts, which if inquired into, would, we doubt not, turn out as groundless as his own attempts at reasoning. Indeed, in glancing over his pages, we have sometimes felt at a loss whether to imagine that Mr Gream *really* believed in what he stated, and was so unacquainted with pathology and medical reasoning as his observations seemed to show; or whether the logical ignorance of the author was not real, but merely affected and assumed for the purpose of enabling him to make broader mis-statements. At all events, this is evident from the pamphlet, that if it is to be taken as a criterion of the arguments that can be used against ether and chloroform, the success of anæsthesia in midwifery is as certain as the success of vaccination in medicine,—seeing that the lovers of pain must be desperate, and that Mr Gream, with all his anxiety to retard and decry the new practice, has not been able to adduce against it a single case or argument that any educated medical man would place the slightest degree of value upon.

The pamphlet, in fact, is only fit (to use his own expressions) for “the hands of persons *not* belonging to the medical profession;” and we owe an apology to our readers for noticing a production like the present at such length. We need only add, that an author who, like Mr Gream, is so defective in other forms of knowledge, is sadly deficient also in the knowledge of his own and of other languages. He writes, for example, more than once, “*ebriety*” for “*inebriety*,”—conjoins plural nouns with singular verbs,—spells forceps “*forcepts*”—sets at defiance all rules of punctuation; and translates the French expression “*salive écumeuse*,” by “*squamous saliva*,” instead of “*spumous*,” “*rushing (raptus) of blood to the head*,” he translates “*ruptures of blood*,” &c. &c. He is, in short, himself apparently much in the state in which he pathetically describes

a patient under ether. "He has undoubtedly lost his reasoning power, for his conduct is most outrageous. He closes his eyes" (*to the truth?*) "and foams at the mouth."—(P. 12.)

Part Third.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXVII.

MEETING XI.—May 17th, 1848.—Dr HAMILTON, President, in the Chair.

ON THE PHYSIOLOGICAL ACTION OF CHLOROFORM. BY DR GUNNING.

THIS paper consisted of illustrations, by experiment and observation, of the theory of the author, that in the mode of action on the nervous and vascular systems, the effects of chloroform were similar to those of a modified asphyxia. The conclusions of the author were as follows:—

1. That there is, after inhaling chloroform and similar substances, an impediment to the transit of the blood through the pulmonary capillaries, which impediment reacts, through the right side of the heart, upon the nervous centres; in which the pressure of the venous, as well as the want of arterial blood, induces the changes which produce the striking effects upon voluntary motion and sensation.
2. That, in all essential particulars, the full effects of chloroform are the same as those of confessedly asphyxiating causes, such as carbonic acid, hanging, drowning, &c.
3. That the same treatment is applicable to those suffering from any of these causes, namely, to renew the relation between the blood in the lungs and the atmosphere by artificial respiration, and to take off the pressure from the right side of the heart by blood-letting.
4. That, therefore, the distinction of substances into "Stimulants" and "Sedatives" is, in many cases, not scientific, seeing they produce similar symptoms and similar material effects.

Dr Simpson said, that he sincerely believed Dr Gunning's theory of anæsthesia to be entirely incorrect; but it was difficult to state, on the spur of the moment, the reasons which might be adduced against so new and strange a view of the subject as Dr Gunning had taken. But the state of anæsthesia was assuredly not a state of asphyxia, any more than the state of narcotism from opium, &c., was a state of asphyxia. Dr Simpson had often in midwifery seen the state of complete anæsthesia from chloroform kept up for one, two, three, four, or more hours, and the person immediately and perfectly recover as soon as the inhalation of chloroform had ceased. If you induced and maintained asphyxia, simple asphyxia, by closing the glottis, compressing the windpipe, or any such direct means, for as many minutes, you would have a very different result. Yet, if Dr Gunning's doctrine were true, and anæsthesia was asphyxia, you ought to be able to induce and maintain it by merely drawing your patient's neckcloth tight for the time being, or by covering over his mouth and nostrils by a handkerchief or towel. Surely Dr Gunning would not venture to reduce his theory to practice. Ville and Blandin, in their experiments on ether, had actually found the elimination of carbonic acid from the lungs increased during anæsthesia, instead of decreased as in asphyxia, and as they *a priori* expected. Dr Gunning believed the more immediate cause of chloroform anæsthesia, was an increased degree of pressure on the brain from impeded return of blood in the jugular veins. But mere increas-

ed pressure on the brain did not produce anæsthesia or coma. Send a man down thirty-four feet in a diving bell, and you have the brain through the blood-vessels (as Dr Neil Arnott has shown), sustaining an additional pressure of fifteen pounds on the square inch; but the man by this—or by a double depth and double pressure to this—is not rendered anæsthetic or comatose. In the experiments performed by a committee of this Society, in November last, on the lower animals, no cerebral congestion was found in any case, and pulmonary congestion in only one or two, by no means in many. The Society were bound to this statement by their own report. *Littera scripta manet.* Nor was there any thing like adequate proof in Dr Gunning's few and imperfect experiments on the lower animals, that the state of anæsthesia essentially produced impediment—as he supposed—in the return of blood along the jugular and other veins to the heart. Some struggling on the part of the animals operated on, would produce the distension of the jugulars which Dr Gunning supposed he had seen. Indeed, that the state of anæsthesia produced the very reverse of this effect—and the very opposite of that which was required by Dr Gunning's theory—seemed shown in a late operation of Professor Miller's. He was removing a tumour from the axillary region of a child; some of the large veins there were opened, but bled little, till the effect of the chloroform accidentally passed off and the child began to cry; then the blood flowed freely; but it became arrested again when the child, by a few inhalations, was again chloroformed. The state of anæsthesia acted like a hæmastic; it should have produced exactly the opposite effect if Dr Gunning's theory had been true. Altogether, the explanation of anæsthesia from nitrous oxide, ether, chloroform, &c., was, he believed, to be sought for in some law of a far higher degree of generalization than that adopted by Dr Gunning. Dr Simpson stated, that he had successfully chloroformed several of the lower animals, fishes, crustacea, annelida, &c., and last year had etherized the sensitive plant (*Mimosa pudica*); and Dr Gunning's theory could not by any possibility apply to some of these beings, where yet the phenomena were essentially similar to what they were in man. He had found, for example, the common earthworm (*Lumbricus terrestris*) very susceptible of the action of the vapour of chloroform. But the phenomena in it could not be explained by impeding jugular circulation, compressed brain, &c., as it had neither jugular vein nor brain. Dr Simpson further stated, that he had found he could, with chloroform vapour, render anæsthetic individual parts of the worm merely, as the tail merely, or the head merely, or the middle part merely, the head and tail remaining unaffected. He could produce thus *local* anæsthesia in the worm by applying the vapour locally. In the human subject, *local* anæsthesia of a portion of the gums could be produced, as he had stated to the Society on a former occasion, by rubbing the part with hydrocyanic acid.¹ These and other circumstances, and the local action of chloroform on plants, seemed to Dr Simpson all to point to the idea, that we are to search for the true explanation of anæsthesia from ether, chloroform, &c., in the direct introduction of these agents into each affected part, either through the blood or juice of the part, or by actual direct imbibition into the tissues.

Professor Miller mentioned the particulars of the case alluded to, in confirmation of what Dr Simpson had stated. It certainly proved the very reverse of Dr Gunning's idea of the state of the large veins, to be true.

Dr Robertson stated a case of convulsions in a child five years old, where the attendant symptoms were those of cerebral congestion, and, after other means had failed, he subdued the convulsions immediately by submitting the child to the influence of chloroform. As the action of the chloroform passed off, the

¹ We are informed by Dr Simpson, that in several individuals he has lately been able to produce a partial, and perhaps superficial, *local* anæsthesia of the human hand, &c., by exposing the part for ten or fifteen minutes to strong chloroform vapour.—Ed.

convulsions returned; and before his patient was quite relieved, and the convulsive tendency passed off, he was obliged to keep up the action of the chloroform for five consecutive hours. Next day he found the child well. He did not think this effect compatible with Dr Gunning's theory.

Dr Skae said, that in asphyxia from carbonic acid, which had been referred to by Dr Gunning as analogous to the action of chloroform, the phenomena had been proved to depend to a great extent upon the narcotic action of the gas, and narcotism differed from pure asphyxia.

A few remarks were then made by Dr Glover, Dr Pirrie, &c., after which, Dr Gunning, in reply to Dr Simpson's remarks, urged, that mere denial of the propositions laid down was little to the purpose, especially as Dr Simpson confessed he had paid little attention to the *modus operandi* of chloroform. The case he alluded to in Professor Miller's practice, where the venous hemorrhage returned after the action of chloroform began to wear off, could not disprove the cases in the rabbits experimented on, where the venous hemorrhage was increased immediately after the use of chloroform. Both might be true, yet not contradictory. Indeed, the objection was rather a confirmation of this view; for while the pulse is quicker and stronger, and the impediment is only beginning, bleeding from the veins should be more ready; but when the action is fully developed, and therefore less blood emitted, that too with less force, from the left side of the heart, it ought to be less, as in Mr Miller's case. Exceptional observations of this kind were of little worth unless all the circumstances were at the same time weighed. He would like to know how Dr Simpson could explain the phenomena in hanging, except upon the view of impediment in the lungs? There is nothing absorbed, and yet there is immediate loss of sensibility and motion, besides all the other effects of chloroform, as extreme pleasure, flashes of light, hissing sounds in the ear, &c. Also, how should artificial respiration, by taking away the arrest at the lungs, so easily restore sensation and voluntary power, if chloroform had some peculiar and mysterious influence upon the nervous matter itself? If the treatment for the extreme effects of chloroform is the same as for asphyxia, if the *post-mortem* appearances are the same, and if the symptoms during life are the same, it is not unreasonable to believe that the *rationale* is also the same. The special provisions for taking off the pressure on the venous system of the cetacea and diving birds, he could not but regard as corroborative of the view he had taken in his paper. If the blood could get easily through them, why such a conformation of the veins? Dr Simpson's other objections, taken from its effects upon plants and animals, he (Dr G.) regarded as irrelevant. That it could not kill by altering the physical relations between the blood and the brain, seeing it killed animals without one—and also plants—was as good as saying, that cerebrate animals could not be killed by concussion, seeing that concussion killed animals without a brain. Dr Skae had reminded the Society that carbonic acid acted as a narcotic poison, but he forgot that narcotic poisons were fatal by asphyxia. If similar symptoms follow hanging, he (Dr G.) preferred believing that the origin of these was at the lungs even when carbonic acid is absorbed.

MEETING XII.—Dr HAMILTON, President, in the Chair.

HISTORY OF A FATAL CASE OF ARREST OF A FOREIGN BODY IN THE OESOPHAGUS. BY ROBERT PATERSON, M.D.—(This case was illustrated by a drawing of the parts).

ON SOLUTIONS OF GUN-COTTON, GUTTA PERCHA, AND CAOUTCHOUC, AS DRESSINGS FOR WOUNDS, ETC. BY PROFESSOR SIMPSON.

At different periods in the history of Surgery, very different practices have prevailed in regard to the treatment of wounds. At one time, in injuries or incisions of any great extent, the whole sides of the divided or cut surfaces

were dressed as separate and distinct wounds; and all chance of immediate union prevented. Slow reunion by suppuration and granulation, or, as the old surgeons termed it, by "digesting, mundifying, and incarning" wounds, was alone attempted. Afterwards, however, and yet not without much doubt and opposition,¹ the practice was introduced of placing from the first the sides and lips of the wound in contact, and thus allowing nature to produce the spontaneous adhesion of the whole wound, or as much of its surfaces as will thus adhere. In other words, reunion by the first intention came to be more and more attempted *after* the discovery of the doctrine of adhesion (as it was termed) was duly made and fully acted on.

But no small difference of opinion and practice has prevailed as to the best mode of bringing and retaining in contact the sides of such wounds as are capable of healing by the first intention. A great variety of bandages, plasters, needles, and stitches have been invented for this purpose. And, the propriety or non-propriety of using sutures in preference to plasters (the *sutura cruenta*, as it was termed, in preference to the *sutura sicca*), was at one time a special subject of dispute. Louis, Pibrac, and Young maintained that in all, or in almost all cases, the employment of the suture should be abandoned as irritating and hurtful. At the present day, both modes of artificial reunion—the *sutura cruenta* and *sicca*—are followed by the generality of surgeons; and often both modes are used simultaneously.

It seems not at all improbable, that another step in advance will betimes be effected, and that surgeons will be enabled to apply to the wound, after its edges are brought in contact, some material or other which, like an artificial plasma, or lute of coagulable lymph, will at one and the same time serve the following purposes:—

1. It will be sufficiently strong and adhesive to retain the edges of the wound together, without the irritation sometimes following the use and removal of sutures or pins;
2. It will serve as a perfect dressing to the wound.
3. It will, however, not be soluble in water or be easily removed, and hence will enable the surgeon to apply cold, &c., to restrain and modify the action in the wound, if required.
4. Though insoluble in water, the material used must be soluble in some menstruum that is easily and readily vaporized.
5. It will be applied in a fluid or semifluid form, and be thus capable of adapting itself to any irregularity in the edges of the wound or in the neighbouring cutaneous surface; the fluid part evaporating speedily on exposure;—and a solid tissue or substance possessing sufficient tenacity, adhesiveness, and insolubility in water for the above purposes, being left like a plaster on the edges of the united wound.

The intention of the present imperfect communication is to bring before the Society some of the attempts lately made in order to attain the above objects.

Two or three weeks ago, it was announced in our weekly medical journals, that a solution of gun-cotton in ether had been used in America as a dressing for wounds; and, within the last two or three days, the American journals which have arrived in Edinburgh, show that the suggestion of this practice is claimed by two different persons, viz., by Mr Maynard, a medical student of Boston, and by Dr Bigelow of the same city.

Soon after the discovery of gun-cotton or pyroxyline, Schoenbein and Boettger showed that acetic ether was one of its best solvents.—(*Chemical Gazette*

¹ "I would ask, (gravely writes O'Halloran in 1765), I would ask the most ignorant in our profession, whether he ever saw or heard even of a wound, though no more than one inch long, united in so short a time (as three days). These tales are told with more confidence than veracity; healing by inoculation, by the first intention, by immediate coalescence without suppuration, is merely chimerical, and opposite to the rules of nature."

for April 1, 1847.) Richner had ascertained that it was entirely soluble in the acetic ethers both of alcohol and wood spirit.—(*Ib.* for February 1.) When xyloidine, or the analogous compound made by the action of nitric acid on starch, is dissolved in ether, or ether and alcohol, Schoenbein found that it was reduced to a colourless gelatinous mass, and that, if spread upon a smooth surface, "*it left a dull white opaque membrane.*"—(*Ib.* for April 1.) I am not aware whether he has recorded any similar observation regarding the ethereal solution of pyroxline; but it is this quality of it that has been turned in America to practical purposes as an application and dressing for wounds.

When gun-cotton is fully dissolved in strong sulphuric ether, it forms a semi-transparent gelatinous pulp. When a layer of this is laid on any surface, the ether speedily evaporates, and leaves an adherent whitish cotton-like web, which contracts strongly as it dries; and possesses still all the usual inflammable and explosive properties of gun-cotton. In a great variety of trials which I have had made as to the best mode of forming the solution, I have met with very various results. Many of these variations were probably owing to imperfections and differences, either in the quality of the gun-cotton or of the ether. But occasionally, with the same ingredients, the quality of the solution seemed liable to differ. Sometimes after the gun-cotton had been immersed in the ether for ten or twelve hours without much effect, the addition of a little strong spirit immediately effected a perfect solution. The gun-cotton often possesses strong adhesive properties when used before it is all completely dissolved. In using as a solvent, aldehyde, which had been kept for a considerable time, I found a perfect pulpy solution formed in a few minutes. If it could be used as a menstruum, it would possess the advantage of being even more vaporizable than ether. But in trying to repeat the experiment with some aldehyde newly formed for the purpose, the same results were not obtained.

Usually an ounce of strong sulphuric ether will dissolve thirty grains or more of gun-cotton in the course of a few hours. But, to form a complete pulp, it will in general require to stand for a day. The advantages which the ethereal solution of gun-cotton seems to possess as a dressing for wounds, and particularly for those which it is desired to unite by the first intention, are stated in the following terms by Dr Bigelow:—

"1st, By its powerful contraction upon evaporation, it places the edges of an incised wound in much more intimate contact than is obtained by sutures and adhesive cloth—unites them by equal pressure throughout the whole extent of the wound, and maintains them immoveably fixed. 2^d, It preserves the wound perfectly from contact with air, being impermeable to the atmosphere, while its adhesion to the skin is so intimate as to preclude the possibility of air entering beneath the edges. 3^d, The substance remaining in contact with the skin and wound after the evaporation of the ether, seems to be entirely inert, so far as any irritating property is concerned, and this can scarcely be said of any resinous adhesive cloth or preparation. 4th, It does away with the necessity for sutures in incised wounds of almost any extent. 5th, It is sure to remain in intimate contact with the skin till union is complete, and being quite impervious to water, and presenting a polished surface, it allows the surrounding parts to be washed without regard to the wound or dressing. 6th, It is colourless and transparent, thus permitting the surgeon to witness all that goes on beneath, without involving the necessity for its removal. 7th, No heat is necessary for its application, and the presence of any moderate degree of cold is only objectionable in retarding the evaporation of the ether. 8th, It may be made at a trifling cost; an ounce phial intrinsically worth little, being sufficient for a great number of dressings."

The adhesive power of this solution of gun-cotton is, when it is properly made and applied, certainly very great. In evidence of its strong adhesive powers Mr Maynard states the following experiments:—"He glued a strap of sheepskin to the hand by a thin layer of the solution, nine lines long and one and a half wide, and it sustained a weight of two pounds. A second strap,

attached to the hand by a layer of the substance, nine lines in length and three in width, sustained a weight of three. A third strap, fixed to the hand by a layer of the liquid, twelve lines square, resisted the force of ten pounds without giving way; and a fourth strap of the leather, glued to the hand by a stratum of the solution, measuring one and three fourths of an inch in length, and one in width, was not separated from its attachment by the gravity of twenty pounds!" These statements, says Mr Maynard, may appear incredible, but they are founded on exact and carefully performed experiments.

As to the mode of applying the ethereal solution of gun-cotton, Mr Maynard states, that, "In slight cuts a moderately thick coating of the solution, laid over the incised parts, was, on becoming dry, sufficient to keep the lips of the wound in position till union took place; but in most instances it was employed in conjunction with straps of cotton and sheepskin, and with raw cotton, forming with them strong, unyielding, adhesive straps, bandages, and encasements; and, after many experiments, I am convinced that this is the best and most effectual way in which it can be employed as an adhesive agent in surgery. The solution dries rapidly, and in a few seconds; by the evaporation of the ether it contains, it becomes solid and impermeable to water. And a strap moistened with it, and glued to any part of the cutaneous surface, adheres to it with a tenacity that is truly surprising." Mr Maynard mentions one case in which, after the removal of a tumour from the scalp, the hair in the neighbourhood was shaved, and two pieces of sheepskin were firmly sealed, by the solution of gun-cotton, to each side of the wound at a distance from the edges, and then these edges were approximated and retained in contact by passing stitches through the dead sheepskin instead of passing sutures through the living tissues of the walls of the wound.

Dr Bigelow's directions for applying the gun-cotton solution are as follows:—"For straight incisions of *whatever length*, provided the edges can be brought together without great difficulty, it is better to apply the solution in immediate contact with the skin, as follows:—The bleeding should be arrested, and the skin thoroughly dried. If the lips of the wound are themselves in contact, the surgeon has only to apply a coating of the solution lengthwise over the approximated edges by means of a camel's hair pencil, leaving it untouched after the brush has once passed over it till it is dry, during, perhaps, ten or twenty seconds. This first film will of itself have confined the edges together; but in order to increase the firmness of the support, more must then be applied in the same manner, allowing it to extend on either side of the incision a half an inch or more. If, however, the wound gapes, an assistant is required to bring the edges in contact, and retain them so whilst the application is made. If the incision is so long that the assistant cannot place the edges in apposition throughout the whole extent, begin by covering a small portion at the upper end, and apply the solution to the lower parts as fast as it becomes dry above. In this case something more than the film which is left adherent to the skin will be necessary for a safe and proper support to the wound, which may have a tendency to separate. The transparency of the dressing may be still maintained by adapting a piece of goldbeater's skin or oiled silk to the wound. This should be covered with the solution, and the membrane applied after the coating is on and already contracted. A dossil of lint, or a strip of cloth, or even a piece of tissue paper, which is thus rendered tough and waterproof, will answer the same purpose, though not transparent. Where there is much separation, it is better to fortify the wound in this way at once, and as fast as the first coating is applied and dry. "If, however, adhesion by first intention be not desired, the gum may be painted on in transverse strips, like adhesive cloth, letting the first strip dry, and giving it the goldbeater's skin support before the second is applied. Thus room is left for the escape of pus, and the exposed portion may be watched without removing the strips."

It has been proposed to use the ethereal solution of gun cotton for other purposes than the dressing and union of wounds, as, for example, as a sub-

stitute for the starch bandage in fractures ; as an application and dressing to ulcers, &c. In abrasions, and slight injuries of the skin about the fingers, it forms an excellent and most adhesive dressing. There is one extremely painful and unmanageable form of ulcer in which I applied it eight or ten days ago, at the Maternity Hospital, with perfect success. I allude to fissures at the base of the nipple. Most practitioners know well the agony that some mothers undergo, in consequence of this apparently slight disease ; the ulcer or fissure being renewed and torn open with each application of the child. In two such cases I united the edges of the fissures, and covered them over with the solution of gun-cotton, making the layer pretty strong. It acted successfully, by maintaining the edges so firmly together that they were not again re-opened by the infant ; the gun-cotton dressing was not like other dressings, affected by the moisture of the child's mouth ;—and as a dressing, and at the same time by securing rest to the part, it allowed complete adhesion and cicatrization speedily to take place.

In a case in which Professor Miller lately removed a large portion of necrosed bone from the lower jaw, I dressed the lips of the incision with the gun-cotton solution, and it held them for some days subsequently accurately in contact till adhesion took place. In two minor amputations (one of the finger and the other of the toe), by the same gentleman, it was applied, but required to be removed in a day or two, in consequence of it retaining the discharges.

I have made a number of experiments, with solutions in different menstrua, of various other substances besides gun-cotton, especially viscine, dextrine, caoutchouc, and gutta-percha.

Gutta-percha readily, I find, dissolves in chloroform. When a thin layer of this solution is spread upon the skin or any other surface, the chloroform rapidly evaporates, and leaves a film or web of gutta-percha, possessing all the tenacity and other properties of that substance. A layer of it, of the thickness of good writing-paper, has perhaps as much strength and tenacity as to hold the edges of a wound together, with all the required firmness and strength of sutures. When a film of it is placed upon the skin, and is allowed to dry thoroughly for a few minutes, the subsequent attempt to separate it, is like peeling and tearing off the epidermis after erysipelas, &c. It then forms, as it were, at once a kind of artificial tissue, epidermis, or skin, which adheres strongly for a time. There is one disadvantage pertaining to it. In the course of a day or two it generally dries and crimps up, like court plaster, at its edges. If we could either increase its adhesiveness, or destroy its tendency to dry and crimp, we would render it more useful. I have seen the addition of a little caoutchouc apparently correct it in these respects.

The deposit or "plaster" left by the solution of gutta-percha is far more equable, smooth, and skin-like than that left by the solution of gun-cotton.

A solution of caoutchouc in bisulphuret of carbon, ether, or chloroform, leaves a very thin, but perhaps less regular web, and one which stretches too readily for most practical purposes.

One great deficit in this class of dressings is the want of a menstruum sufficiently powerful, and, at the same time, not stimulating like ether or chloroform. This objection may, perhaps, in practice be got over, by applying an unstimulating solution of isinglass or the like, to the raw surface, before applying the stimulating solution of the gun-cotton or gutta-percha. Or the first layer of gun-cotton or gutta-percha may be made very thin, so as to evaporate almost instantaneously, and then afterwards a series of superincumbent layers may be added till the web is of the required strength. Other better substances for solution may, perhaps, be found. But no material has a chance of succeeding, unless it be insoluble in water after it is consolidated, and unless it be sufficiently strong in its texture, and possesses powerful adhesive properties.

[A variety of specimens, showing the kind of artificial tissue made by the above and other solutions, was shown by Dr Simpson to the Society.]

REPORT ON THE EMPLOYMENT OF CHLOROFORM IN MIDWIFERY, ETC.

Dr Simpson gave a long report and detailed communication on the employment of chloroform in midwifery, stating that he had used it constantly, and with the best results, in his own practice since November; mentioning the rules required to be attended to in its exhibition; answering the supposed objections to its use, &c. &c. He read numerous communications and reports regarding its employment, from *Dr Grigor* of Nairn, *Professor Dyce* of Aberdeen, *Mr Lawrence* of Montrose, *Dr Paton* of Dundee, *Dr Anderson* of Glasgow, &c. &c., showing that a great number of persons had been already successfully delivered without pain or suffering under the use of chloroform during the last six months. (*Dr Simpson's* communication will be published in our next Number.)

Drs Moir, Malcolm, Leith, Carmichael, &c., stated to the Society, some verbally, and others in writing, the uniform and successful results which they had met with, employing it, as they did, constantly in their practice, and in all cases of labour.

Mr Crisp of London stated, that though a stranger, he was induced to rise were it for no other purpose than to say, that, after having attended the meetings of many a medical society, he had never till to-night seen one that was unanimous in opinion on any topic. He had come to Edinburgh a fortnight ago, and now entertained a totally different opinion about chloroform from what he did when he arrived; for he had now seen it constantly and most successfully employed in the hospital and elsewhere. At the same time, although this had been the result of additional experience on his own mind, he was not disposed to blame, but, on the contrary, to commend the scepticism which had been shown by many most eminent men in London and elsewhere, on this subject, which he thought was no more than justified in relation to an agent of such a novel kind, and so important in its practical application. He believed that this scepticism had not its origin in any exclusive or bigoted feeling, but would be overcome as soon as the facts came to be as well known in London as they are in Edinburgh.

Dr Bennett considered it probable, that one of the reasons chloroform was not much used out of Edinburgh, was the impurity of the article administered. It was not long ago that *Dr Clay* of Manchester had stated to the Society, that although he had frequently seen it given in that town, he had never witnessed its proper effects produced until he came to Edinburgh. *Mr Crisp* from London had just made a similar statement. *Dr G. Wilson* had lately informed him, that even the chloroform manufactured in Edinburgh was not so pure as it might be, and that he had lately purified some which produced the full effect more rapidly, and with a smaller quantity, than that in ordinary use. He (*Dr B.*) conceived that the purity of the chloroform was not sufficiently attended to by those who had tried it, and that those who would not, had better, like *Dr Clay* and *Mr Crisp*, come and see it given in Edinburgh.

Dr Simpson observed, that he believed the want of success in England was owing also to another cause. From what he had learned, he was quite convinced that our English brethren, in using chloroform, often stopped altogether at that point which really constituted the true commencement of the effects of the inhalation. Immediately before the chloroform produced anaesthesia, more especially if there was any noise or disturbance, it not unfrequently excited the patient, who would talk incoherently for a moment or two, beg the inhalation to be suspended, perhaps struggle to get free from it, and have his arms and legs thrown into a state of strong clonic spasms. In Edinburgh, we all sufficiently know that these symptoms indicate merely that the patient is about to come under the full influence of the vapour, and that, in a minute or so, these symptoms will pass, and he will immediately be completely anaesthetic and completely unconscious. But in England these premonitory symptoms seem to have been often regarded as very alarming, and all attempts at further

inhalation stopt, exactly where and when the dose of the vapour should have been increased. And in the English journals such cases have been repeatedly and gravely recorded as instances of delirium, and spasms, and convulsions, and failure. They are not more anxious, or deserving of attention, than the same symptoms would be in a case of hysteria, and are quite transient if the inhalation is only persevered in. Dr Simpson added, that now, amongst many hundred patients, he had never yet met with one instance in which any person was insusceptible of the full effects of the chloroform. He knew that the experience of many of his brethren around him went to the same effect. Nor, in any one case, had he seen any marked bad effect from the full use of the chloroform. Deaths will occur after operations, and sometimes even during them; but every death during an operation was not, as some of late in the south have argued, from chloroform. A gentleman near him, Dr Paterson, some weeks ago opened with his lancet a large abscess in a child's neck. There was no hemorrhage; but in a minute or two, at most, after the incision was made, the child was dead. An English jury might possibly have anxiously tried to bring it in as a case of death, probably from chloroform. But it certainly was not so, for the very simple reason, that no chloroform whatever was used, the incision being considered too slight to require it. Dr Simpson asked Professor Miller and Dr Duncan to state the extent to which they used chloroform in their public and private surgical practice.

Professor Miller observed, that in the hospital and elsewhere the surgeons of Edinburgh had used chloroform in all their operations, with the exception, perhaps, of any such within the cavity of the mouth as were expected to be attended with much hemorrhage. And he could speak of its perfect success, and perfect certainty, and perfect safety, in the most unequivocal terms. There had been no misadventures, no failures, and now no fears of those spasms and other preliminary symptoms to which Dr Simpson had alluded. In saying all this, he believed he was simply stating the opinion and experience of all his surgical brethren here; and that no one amongst them would deem himself justified, morally or professionally, in now cutting and operating upon a patient in a waking and sensitive state. Every professional principle, nay, the common principles of humanity, forbid it, seeing that surgery was now happily possessed of sure and safe means by which it could avoid the necessity of such cruelty. These were strong opinions, strongly expressed, but, in answer to Dr Simpson's question, it was impossible for him to say less.

Dr Duncan stated, that he sincerely coincided in every part of the statement made by Professor Miller, and that, in his hospital and in his private practice, he constantly, like his other surgical brethren, used chloroform in all his operations, and even when making any painful examination for the purpose of diagnosis. There was only one case in which he had found a difficulty in its application, viz. when operating for internal hemorrhoids, the patient not, of course, having the capability of protruding the bowel when anæsthetic.

Professor Miller stated that, in operating for internal hemorrhoids, he had latterly been in the habit of making the patient first protrude the bowel; he then fixed the hemorrhoid with a vulsellum, chloroformed the patient, and afterwards terminated the operation.

TRIAL OF DR GIBSON FOR CULPABLE HOMICIDE, ETC.—(Continued from last No.)

THE medical witnesses for the defence were Drs Couper, W. Thomson, Lawrie, and Rainey of Glasgow, and Drs Alison, Christison, J. Y. Simpson, and Scott of Edinburgh; only three of these, however, were called into court.

Dr Couper, is Professor of Materia Medica in the University of Glasgow, and has been for some time in considerable practice as a medical man. Has known Dr Gibson for a long time. He has been regularly educated, and holds a high place in his profession. Had heard the report of the *post-mortem* examination of M'Ginty. From that examination, could not say with any certainty whe-

ther the disease of the peritoneum could have been apparent on the 19th of January? it is probable, but not certain. Would not have suspected disease of the peritoneum, if, on the 18th, the symptoms had been emaciation, diarrhœa, cheerfulness of spirit, debility, but not preventing the patient from walking, and if disease of the lungs had been clearly manifested. He would have concluded that disease of the lungs was sufficient to account for these symptoms. Judging from the account of the examination he had heard, considered that the immediate cause of death was tubercular peritonitis, conjoined with disease in the glands, which again followed disease of the lungs. Such are common complications of consumption. The symptoms they occasion are very often deceptive, and difficult to discover. One of the symptoms is distension or swelling of the abdomen. In the more advanced stage this may depend on effusion of fluid; in the earlier stage it might be owing to the distension of the bowels with air, or both may be combined. In the case of effusion of fluid, it sometimes comes on very rapidly. It may come on in two or three days, and it may give rise to distension or swelling of the abdomen, which was imperceptible before. It is possible that such swelling might have taken place during a sea voyage of two days' duration. Besides the distension, the other symptoms of peritonitis are pain, disturbance of the functions of the bowels, fever, debility, and emaciation.¹ Of these, pain on pressure is the most characteristic symptom. The other symptoms may accompany disease of the lungs, independent of tubercular peritonitis. It may be conceived that chronic peritonitis might be stimulated into activity by disorder of the bowels from eating fruit. It is quite possible for the disease to exist for a considerable time without being discovered. It occurs often. If the appetite had been good before the voyage, and bad after it, would conclude that the disease had undergone some alteration in the interval. Would not consider a person labouring under the symptoms described, to undergo any danger from a voyage to London. Not even in the month of January, if due care be taken. Sea voyages have been recommended to consumptive patients. The care necessary to be taken, should consist of a conveyance in a close carriage on land, warm clothing, and remaining between decks at sea, to avoid cold air and damp. Had the symptoms been such as have been described, would have signed the certificate.

Cross-examined by the Advocate-Depute.—Understood that swelling of the abdomen was excluded from the symptoms mentioned. Did not consider diarrhœa an objection to the voyage, unless it was very severe. Supposing both swelling of the abdomen, and fluctuation on pressure to have been present, much would depend upon the amount of pain and amount of swelling. Would still feel at liberty to sign the certificate, although a certain amount of pain and swelling had been discernible. A person under such disease would not recover under any circumstances. It was a mere question of place, whether he was to die in Glasgow or at Millbank.

Re-examined by Mr Moncreiff.—If Millbank was a better residence than Glasgow, removal would be beneficial.

Examined by the Lord Justice-Clerk.—If the patient had been confined from the 20th of December, with a pulse varying from 108 to 120, no appetite, taking pills to stop diarrhœa, thought he might still be conveyed to Millbank without injury. He had ordered patients under similar circumstances to be removed in the month of January. Recollected one case where, under his sanction and by his advice, a gentleman was removed several miles by land, to a ship going to Madeira. That was in 1829, and he is now alive and well.

Dr William Thomson is Professor of the Practice of Physic in the University of Glasgow. Is acquainted with Dr Gibson, who stands very high as a medical man. He has been highly educated, and is at present one of the directors of the Glasgow Infirmary. From the account of the *post-mortem* examination,

¹ Here the symptoms of acute peritonitis, owing to the mode of questioning by Counsel, became confounded with those of tubercular peritonitis.—Ed.

considered that M'Ginty died of complicated tubercular disease of the lungs and of the belly; but, in all probability, the immediate cause of death was the disease in the belly. Could not tell how long it had lasted; sometimes such diseases are very slow in their progress, at others very rapid. In its early stage, it is often exceedingly slow and latent. It frequently happens that the disease remains latent for a considerable time, and then develops itself rapidly. The disease in the belly is often distinguished by tympanitic swelling, and swelling from effusion. Such swelling might undergo a very considerable alteration in twenty-four or forty-eight hours, particularly if caused by effusion. In many cases there is a total absence of pain, and sometimes it is very painful. Emaciation, quick pulse, and diarrhoea, are independent of abdominal complication. Would not consider a person labouring under such symptoms, unfit to take a journey by sea from Leith to London; sea voyages are often recommended, and have been proved to have been beneficial in cases of advanced consumption. His signing the certificate would depend on circumstances. If required to sign the certificate for the purpose of transmitting a convict without danger to his health, would have thought himself bound to do so.

Cross-examined by the Advocate-Depute.—If there had been no requirement to transmit, would have said it was better for him to remain.

Examined by the Lord Justice-Clerk.—Supposing even that the symptoms were worse than had been stated, and that abdominal swelling were present, would not consider granting the certificate was such an improper act, that the person was very culpable for granting it; thought there was a latitude allowable. Did not consider there were any facts in this case which would justify a medical man in considering the voyage to be wrong. Death might be protracted, or advancing.

By Lord Cockburn.—But, supposing it wrong, there was certainly nothing so outrageously wrong as to exclude the idea of the opinion in that certificate being held by a respectable medical man.

By the Lord Justice-Clerk.—He might be wrong or not, but there was nothing he had heard that enabled him to say, that, professing to be a medical man, he might not honestly sign that certificate, and hold that opinion. It is much easier looking retrospectively.—(Here witness was interrupted by the bench.) When patients obtain any thing they imagine will do them good, they sometimes rally. The mental state may easily be conceived to have a very considerable influence on the progress of the disease. If the patient was anxious to be removed, thought he might be indulged, especially if the point were difficult to determine. If the lungs were primarily diseased, it might account for the attention of a medical man not being so much drawn to the abdomen. Had often found that, by the patient directing his attention to new things, he has discovered what at first had escaped observation.

Professor Christison had heard the account of the *post-mortem* examination. The cause of death was the complicated effect of pulmonary consumption, ulceration of the bowels, and chronic inflammation of the external membrane of the bowels. Could not tell how long symptoms of the chronic inflammation had existed. The disease in the lungs was the primary disease; it was of the older standing. It is not frequently accompanied by chronic peritonitis, but frequently with ulceration of the bowels. Chronic peritonitis frequently exists in a latent state. It frequently advances with no signs, and frequently with such slight signs as will not attract attention for a long while. The patient died on the 10th of February, and it is perfectly possible that the perceptible symptoms might not have developed themselves on the 18th of January, although the disease must have existed prior to that. Thought that a medical man was warranted in signing a certificate on the 18th of January, that the party was fit for a sea voyage. There is a difference of opinion as to how far a sea voyage is advantageous in consumptive cases; some think there is a positive advantage in that mode of carriage. Diarrhoea is one of the symp-

toms of pulmonary consumption in the advanced stage ; also high pulse, progressive emaciation, and loss of strength. Is acquainted with the regulations of the jail in Glasgow. Knows there is not that accommodation there which a person in such a hopeless state would require.

Mr Moncreiff stated that he had several other medical witnesses waiting, but thought it unnecessary to call them into Court.

Mr Crawford, the Advocate-Depute, made a very temperate and able speech to the jury, stating that they must be satisfied that this was a case which, from its importance to the public, and to the individual at the bar, ought to be fully investigated. He did not feel justified in asking for a verdict of culpable homicide, or for granting a certificate knowing it to be false. On the third charge, of neglect of duty in a public officer, however, he had reluctantly come to the conclusion that it ought not to be withdrawn. It was no part of the punishment of the guilt of M'Ginty, that in any respect his life should be trifled with ; and if he did not receive as much attention as he would have done in a very different position of society, then there was neglect of duty. The law for the protection of persons in M'Ginty's situation has required, not only that there shall be a medical certificate that he can be removed without danger, but that the certificate shall be granted by a public officer, the particular medical man who has had charge of the criminals. It was therefore the bounden duty of a person holding the situation of surgeon to the jail, to form the best opinion in his power, according to his opportunities, his skill, and his judgment. Now, this unfortunate man M'Ginty had been proved to have been labouring under symptoms which indicated the presence both of pulmonary and abdominal disease. Whether it reached the height in Glasgow which it was afterwards discovered to exist in London, is another question. But it is worthy of remark, that on one side there is the evidence of two medical men who saw M'Ginty, who have drawn their deductions not from theory or notes, or at second hand, but from their own personal observation of the symptoms when the patient was living, and of the appearances found in his body when dead. There is a third medical man confirming that opinion by strong testimony. On the other hand, there is high medical evidence ; but still the evidence of gentlemen who never saw the man. It is for the jury to say, whether, weighing the evidence of those who saw, against that of those who did not see him, they will agree with one or the other on the propriety of his removal. Now, that he was not fit to be removed was beyond all doubt. No one will maintain that, because he was dying, it was of no consequence where he died. There is the most conclusive evidence that the symptoms were discernible on the 21st of January, and it was for the jury to decide whether any grounds existed for supposing such symptoms arose between his removal on the 19th from Glasgow, and his inspection on the 21st in London.

Mr Moncreiff then addressed the jury for the accused, in a speech which evidently made upon them a strong impression. He did not complain of his learned friend for bringing the case to trial. Although it was a great misfortune to his client, he made no complaint. The situation in which Dr Gibson stood was one to which the subjects of every free country may be exposed ; his conduct was open to investigation, and the public prosecutor has chosen to have it in this shape. He thought, however, that his learned friend should, after having given up the two serious charges, have taken a step in advance, and withdrawn them all. He would be willing to stand on his speech, and say nothing more ; but, with the interests of Dr Gibson confided to him, he could not treat the case so lightly. Here, after eulogising Dr Gibson's character and skill as a medical practitioner, he entered at some length into the circumstances which we formerly explained regarding the change of certificates. He contended that it was only Dr Gibson's duty to sign the certificate, which, under the circumstances, any man might have done ; Drs Couper and Thomson would have done it, and here Dr Gibson's duty ended. He was bound to do his duty carefully, and give a medical opinion honestly ; but should he be

told, that, although he may have been mistaken altogether, he is to be placed as a culprit at the bar? Dr Couper said not only that the certificate was honestly signed, but that it might be truly signed, and that M'Ginty's health would not be endangered. That is not to say he was in good health. Dr Gibson never imagined that this man was in good health; but what he held, and the medical men confirm his opinion, was, that a man might labour under this disease, and not have his life endangered by the removal. In Glasgow he was pining in a wretched atmosphere, and longing to get away. One of the wardens said, that the disappointment of being detained would have killed him. He thought the milder air of the south would do him good; and under these circumstances, if the state of the weather had been propitious, and his condition somewhat different, he might have been a gainer and not a loser. The eminent Glasgow professors had said, that in the more advanced stage of pulmonary consumption, a sea voyage may be beneficial, and Dr Couper had given a case where it was attended with great benefit. They also stated that chronic peritonitis might in forty-eight hours undergo such a change, that the case may pass from a latent into such a one as presented itself on his arrival at Millbank. Such he believed to have been the case, caused probably by the fruit M'Ginty had eaten, or by one of those dreadful turns that these maladies often take before their fatal termination. Dr Gibson's character, therefore, as a man of truth comes unscathed out of this inquiry. But it is said there was neglect; now, Dr Gibson's assiduity was unsullied by the evidence. There was not a pretence for saying, that Dr Gibson was not in the jail morning, noon, or night, whenever his duty called him. He stripped the man. How, then, did he neglect his duty? His learned friend had said that he must have found the swelling; but he did not find it, and it is not sure that he must have found it. Every day, things that doctors say will happen, do not happen, and what they say will not take place, do take place. The tenure of many professional men's eminence would be very slender, if, on the mere possibility of their opinion being wrong, you were to consign them to jail. He was not defending Dr Gibson's professional character, that was unnecessary; he was defending him from crime. He thought the jury would be satisfied that Dr Gibson had already suffered enough anxiety from what was no fault of his.

The Lord-Justice-Clerk then summed up the evidence at considerable length, and succinctly placed all the facts of the case before the jury. With regard to the medical evidence, he asked—Might it not happen that the coughing and other symptoms might have led Dr Gibson to confine his attention to the lungs, where the disease first appeared; and his attention not being called by the man to the abdomen, he might not have thought that there was a disorder going on there which would have rendered his removal more difficult? This might lead to a complete explanation of the case without any reflection on Dr Gibson, and which we must be prepared to lay aside before we can come to the conclusion that there was neglect of duty. Other theories have been put forward. It is said that an effusion may have supervened during the voyage, and brought the abdominal disease into a degree of activity not previously discernible. But the gentlemen who examined the body gave evidence against that notion, when they say there were no appearances of recent inflammation. But, supposing Dr Gibson's attention had not been called to it, that is a complete answer to the charge of neglect. One witness had remarked, that it was easy to give an opinion retrospectively, but Dr Gibson made such an examination as from his knowledge of the circumstances he thought most proper. The jury had to consider whether he was culpable in not discovering the abdominal disease, when Dr Thomson had stated that cases have occurred where symptoms might escape observation, from the patient not calling attention to them. The essential part of the case, however, was, whether such culpable neglect had been exhibited, as in the language of the indictment tended "to the detriment of the public service, and the serious injury of the health of the lieges." Is it proved that this man did

suffer seriously by the voyage? No facts have been brought forward by the public prosecutor that he was injured. The medical men of London are most anxious to make out that the disease must have been as fatal on the 18th of January as on the 10th of February, the day of his death. There may have been terrible discomfort to this poor creature, but on arriving at Millbank his condition was not worse than when he left Glasgow. The charge of neglect is said to have been of such a kind as produced injury to the health. This had not been proved. He did not think the jury could lay hold of a fact which tended to show that his health was really endangered. The charge tried was, that there had been culpable neglect of duty, "to the detriment of the public service, and the serious injury to the health of the lieges." Unless that fact be proved, the prisoner at the bar is entitled to a verdict. The inquiry which had taken place was a most important one; the great end of the trial had taken place, and that was quite consistent with a verdict in favour of Dr Gibson.

The jury unanimously¹ and immediately returned a verdict of not guilty. The trial excited great interest among the profession, and the court was crowded by practitioners and students of medicine.

This trial has brought forward several points of the greatest interest to the profession, which we feel called upon shortly to notice.

In the first place, it seems extraordinary that the public prosecutor should bring Dr Gibson to trial for such grave offences as culpable homicide, granting a false certificate knowing it to be so, and neglect of duty as a public officer, when he must have known that the proper certificate had not been sent for signature to Glasgow. Whose duty was it to forward that certificate, and why had it not been sent to Glasgow, as well as to Lanark and other towns? Here undoubtedly was neglect of duty in a public officer, and, in our opinion, it was the person who made this serious error who should have been tried, rather than Dr Gibson.

In the second place, it must be evident that no medical man can ever be safe in this free country, so long as coroners permit juries to throw unfounded aspersions on the characters of medical men. In the judgment lately given by Mr Justice Coleridge, in the case of Dr Berncastle, it is stated that a coroner's jury were empanelled to inquire into the cause of death, and that in throwing imputations on the medical man, when he had no hand in producing the death, they were exceeding their jurisdiction. The verdict in that case was, "That Elizabeth Hopkins had died from exhaustion from protracted labour, and that Dr Berncastle had shown the greatest inhumanity in leaving her, and they thought him unfit to remain as medical officer of the union." The facts of the case were, that Dr Berncastle was not bound to attend the woman without an order; that he sent a person to the proper authority to procure that order, which was refused, and then he left the woman, his professional engagements calling him elsewhere. The jury, however, instead of charging the parish officer with inhumanity for not granting the necessary order, charge it upon the medical man, who loses his situation in consequence. When Dr Berncastle made an application for a *certiorari* to bring up the coroner's inquisition for the purpose of its being quashed or traversed, it was refused by Mr Justice Coleridge on the ground that there was no defect, or no material fact which it was desired to traverse. Now, the case of Dr Gibson is somewhat, though not exactly, similar. Here the verdict was, "That the deceased died from natural causes, but that the surgeon of Glasgow jail ought not to have permitted the deceased to take the journey, and that death was accelerated by the journey." This, which in England amounted to a verdict of manslaughter, fortunately for Dr Gibson, did not lead to the loss of his situation, but to a criminal trial, in which his complete innocence, and the absurdity of the

¹ Our English and Irish readers should perhaps be informed, that a Scotch jury consists of fifteen persons, the majority of whom give the verdict.

charge, was full exposed. At the same time it cannot be denied, that such charges are highly detrimental to the medical men concerned, and that coroner's juries should not be permitted to form such verdicts without a much fuller investigation than is usually had recourse to. Cases of consumption are continually happening in Scotland, where persons are sent to various distances advisedly, in a much worse condition than M'Ginty was. Yet should such persons die in England, say *en route* to Madeira, should any suspicion lead to a coroner's inquest, and on dissection organic disease to be discovered sufficient to account for death, surely it is monstrous to suppose that the medical man is to be tried for culpable homicide, because an ignorant jury, even though supported by too confident and ill-informed medical men, are of opinion that the journey ought not to have been taken. As was well stated by Mr Moncreiff, the tenure of a professional man's eminence would be very short if you consign him to jail every time he forms a wrong opinion, although in the case of Dr Gibson even this was not proved.

Turning now to the case of the man M'Ginty, we have some difficulty in ascertaining the facts which ought to guide our opinion. Instead of an authentic record of the *post-mortem* examination drawn up on the spot, and signed by the medical men present, we have three different accounts. Two of these were said to be derived from notes, the third was stated in court altogether from memory. The accounts, although they of course agree as to the general appearances, differ in some important particulars. But, what is singular, although they differed as to facts, the London medical men all agreed, that under the circumstances a sea voyage would be highly dangerous to the man's life. This opinion, given by all three in the most confident manner, was proved to be utterly erroneous, as M'Ginty arrived in London not one jot worse than when he set out. The Lord Justice-Clerk commented on this point with great acuteness, and told the jury that there was not the slightest evidence that any one symptom he laboured under had been increased by the removal.

The disease M'Ginty laboured under, was phthisis pulmonalis, complicated with tubercular peritonitis, and ulcerations of the intestines. The cause of death was, as stated by Dr Baly, exhaustion caused by these complicated lesions. Here it is necessary to remember, that before leaving Glasgow, Dr Gibson had very judiciously placed his patient on an animal diet, and given him cod liver oil with expectorants and anodynes. On arriving in London he was placed on a farinaceous diet, and afterwards had steel pills as a tonic and astringent. We firmly believe that this practice increased the man's weakness, and hastened his death, although we should regret to see Dr Baly tried for manslaughter for so doing. The more immediate cause of death, however, seemed to be something acute; for Mr Hall the house surgeon tells us, that on the evening previous, he complained of pain in his abdomen, with sickness and vomiting. Now, these are exactly the symptoms of intestinal perforation. On examining the body, the peritoneum was covered with lymph and tubercles, causing adhesions between their opposed surfaces. This is said to have been chronic, but the physical characters of the lymph exuded are nowhere described. There was serum in the abdomen, the amount of which was never stated—a very important omission. This, according to Dr Basham and Mr Hall, was turbid and of a dark dirty colour; but according to Dr Baly, was of a straw colour and perfectly clear—a most unfortunate discrepancy. One ulcer in the intestine had gone through all its coats, although no actual opening could be discovered after death. We think it most probable, however, that a minute perforation had taken place on the evening of the 8th, which caused the sudden pain, sickness, and vomiting, although that perforation may have been blocked up with lymph afterwards. On no other supposition can these symptoms be explained; and of the two appearances described we believe turbidity of the fluid to be the correct one, although from the contradictory evidence, and loose manner of recording the results of the examination, this cannot now be positively ascertained. We are inclined then to believe, that

acute peritonitis supervened upon the chronic, owing to intestinal perforation, causing the fatal symptoms under which in his exhausted condition he sank, and also causing the turbid, dirty fluid to be effused in the peritoneal cavity—by no means an uncommon termination of the disease.

The explanation we have advanced, however, is opposed by the testimony of Dr Baly. That gentleman positively asserted that he detected fluctuation of fluid in the abdomen, on the day of M'Ginty's arrival. Now, without wishing to impugn Dr Baly's accuracy, every practical man must be convinced it is not so easy to detect fluctuation of a small quantity of fluid in the abdominal cavity, especially when the peritoneum is loaded with chronic tubercle. We presume that the quantity was small, because if it existed to any amount, surely so important a fact would not have been omitted. But we have the positive statement of Dr Gibson, who examined the abdomen on the 18th, that there was no fluctuation, only tympanitis, to which he ascribed the swelling. The doughy feeling of tubercular peritonitis might have been mistaken for that of fluctuation, even by the hands of Dr Baly. It is certain, however, that he honestly conceived fluid to be present, and, to explain its existence at that early date, tells us afterwards, that to the best of his belief (for he had no note on *that* point) that the fluid found was ascitic, that is, of pale straw colour, and clear. His colleagues, however, declared that it was turbid, opaque, and dirty, which, as we have above endeavoured to show, corresponds with the symptoms of perforation experienced on the night previous to death. We have, therefore, strong doubt as to the existence of abdominal fluctuation on the day of M'Ginty's arrival. Dr Baly may at that time have strongly suspected fluctuation, and this suspicion might have been confirmed in his mind by the results of the *post-mortem* examination.

Such being the doubts and difficulties of the case, even now, when all the facts in connexion with it have been well scrutinized and weighed, we need not point out how exceedingly unjustifiable were the strong and positive assertions of Drs Baly, Basham, and Mr Hall, as to the time the disease had existed, the facility of detecting it, and the danger to which a short sea voyage exposed the man who laboured under it. We have already seen that no danger *did* arise, and, as a matter of opinion, the medical evidence for the defence is directly contrary to that given for the prosecution. Indeed, we believe there are few medical men of eminence who would not have met such unfounded and crude assertions with a direct denial. The following are some of these assertions, made by Dr Basham. "The cause of death was tubercular peritonitis." "A person with such a disease certainly could not be removed from Glasgow without danger." "The swelling of the abdomen arises from the absorption of lymph, and from the effusion of fluid of a very peculiar character." "In scrofulous peritonitis it is almost impossible for the serum to form in the abdomen suddenly—it is contrary to the nature of the disease that pure serum should be evolved, and, except pure serum, did not think there would be any sudden effusion in the course of a few days." "It is rare for an acute attack of peritonitis to supervene upon chronic peritonitis." "He decidedly thought that, if tubercles existed in the lungs alone, not to speak of their presence elsewhere, it would be most hazardous to send a man a voyage in that season of the year." These assertions, and in some instances extraordinary statements, while they could not fail to excite the astonishment of every well-informed practitioner present, were delivered with a quickness and decision, well calculated to impose upon the non-professional public, and the superficial.

An attentive perusal of the medical evidence in this case, both for and against the accused, must we think convince our readers, that the opinions of the London practitioners were far too positive and decided, and that in stating them in so unqualified a manner, especially as regards the existence of obscure symptoms for a long period, and the danger of a sea voyage, they were by no means warranted by the present state of our knowledge. This is the more to be regretted when it is remembered, that the object of these statements was to

criminate a medical brother, whom no reasonable man could for a moment believe was guilty of the offences ascribed to him.

Notwithstanding the length of these remarks, we cannot dismiss this case without observing that it constitutes no excuse for an imperfect diagnosis, that the attention of the medical man was not directed towards this or that organ. The Lord Justice-Clerk in his remarks to the jury seemed to think that if this explanation was accepted, no blame could be attached to the prisoner. Now, with all deference to the learned judge, we are of opinion that if this had been proved, the accused would necessarily have been guilty. No doubt medical men often do make mistakes from hurry or carelessness, but under such circumstances they neglect their duty; which is, carefully to enquire into the state of *all* the organs and *all* the functions, in order to ensure accuracy as far as the art will permit. We are disposed to believe, that Dr Gibson had done this. Of course, some men are more skilful in forming a correct diagnosis than others, and it is impossible to fix a standard which shall declare when culpability commences, and ability ends. It would never do, however, to maintain, that because some men have overlooked important symptoms, that any one can be excused for having done so.

OPIUM-EATING IN ENGLAND.

From the accounts of the Board of Trade, of the imports and exports of the United Kingdom for the month ending May 5, it would appear that the consumption of opium in this country is greatly on the increase. The quantity imported during the month amounted to no less than 7·029 pounds to 3·083 pounds imported during the same period in 1847. The total quantity of opium imported in 1847, amounted to 24·929 pounds. It is reported that the inmates of our workhouses are given to the practice of opium eating.—*Medical Gazette*.

RECOGNITION OF HUMAN SKIN AFTER THE LAPSE OF CENTURIES.

At a late meeting of the Microscopical Society of London, Mr John Queckett read an interesting paper on the importance of the microscope in the determination of minute structures of a doubtful nature. The author stated that his object in bringing the communication before the Society was to point out how minute portions of skin, which had been exposed to the air for centuries, could be recognised as human. There existed in this country certain traditions, that persons who had committed sacrilege were flayed, and their skins nailed to the doors of the churches they had robbed, and three portions of such skin had been forwarded to the author for examination, by Albert Way, Esq., the secretary of the Archæological Society. The first was taken from one of the doors of Worcester Cathedral; the second specimen from the church door of Hadstock in Essex; and the third from a church door in Copford, also in Essex. On all the specimens, Mr Queckett succeeded in finding two or three hairs, which the microscope clearly proved to be human. Thus, this valuable instrument is able to confirm a tradition, and prove the former prevalence of a practice which had been doubted by many archæologists.—*Lancet*.

RETURN OF MR SYME TO EDINBURGH.

MR SYME returns to Edinburgh in the first week of July. We understand that his resignation of the Chair of Clinical Surgery has been withdrawn, and he resumes his duties in the University and the Infirmary as formerly. The most incorrect information has been circulated by the London weekly press, regarding the movements of this gentlemen, and the proceedings in Edinburgh with reference to the Chair of the Institutes of Medicine.

BOOKS RECEIVED.

1. On the Nature and Treatment of Stomach and Renal Diseases, &c. By William Prout, M.D., F.R.S., &c. The Fifth Edition. London. 8vo. 1848.
2. A Treatise on Diet and Regimen. By William Henry Robertson, M.D., &c. Fourth Edition. Vol. II. London. Small 8vo. 1848.
3. On Wounds and Injuries of the Chest, &c. By G. T. Guthrie, F.R.S. London. Royal 8vo. 1848.
4. Insanity tested by Science, and shown to be a Disease rarely connected with Permanent Organic Lesion of the Brain, &c. By C. M. Burnett, M.D. London. 8vo. 1848.
5. A Descriptive Catalogue of the Anatomical Museum of the Boston Society for Medical Improvement. By J. B. S. Jackson, M.D., Boston. 8vo. 1847.
6. A Descriptive Catalogue of Wet Preparations, Casts, Drawings, Models, Books, &c., contained in the Museum of the Birmingham and Midland Counties' Lying-in Hospital and Dispensary, &c. By John G. Connor, A. C., &c. Birmingham. 8vo. 1847.
7. On Functional Diseases of the Liver, associated with Uterine Derangements, &c. By Butler Lane, M.D., &c. London. 8vo. 1848.
8. An Essay on the Epileptic Form of Puerperal Convulsions, &c. By Joseph Thompson, M.R.C.S., &c. Nottingham. 12mo. 1848.
9. Hospital Elections and Medical Reform, addressed to the Governors of St. George's Hospital and to the Profession. By Edwin Lee, &c. London. 8vo. 1848.

THE FOLLOWING JOURNALS HAVE BEEN RECEIVED IN EXCHANGE.

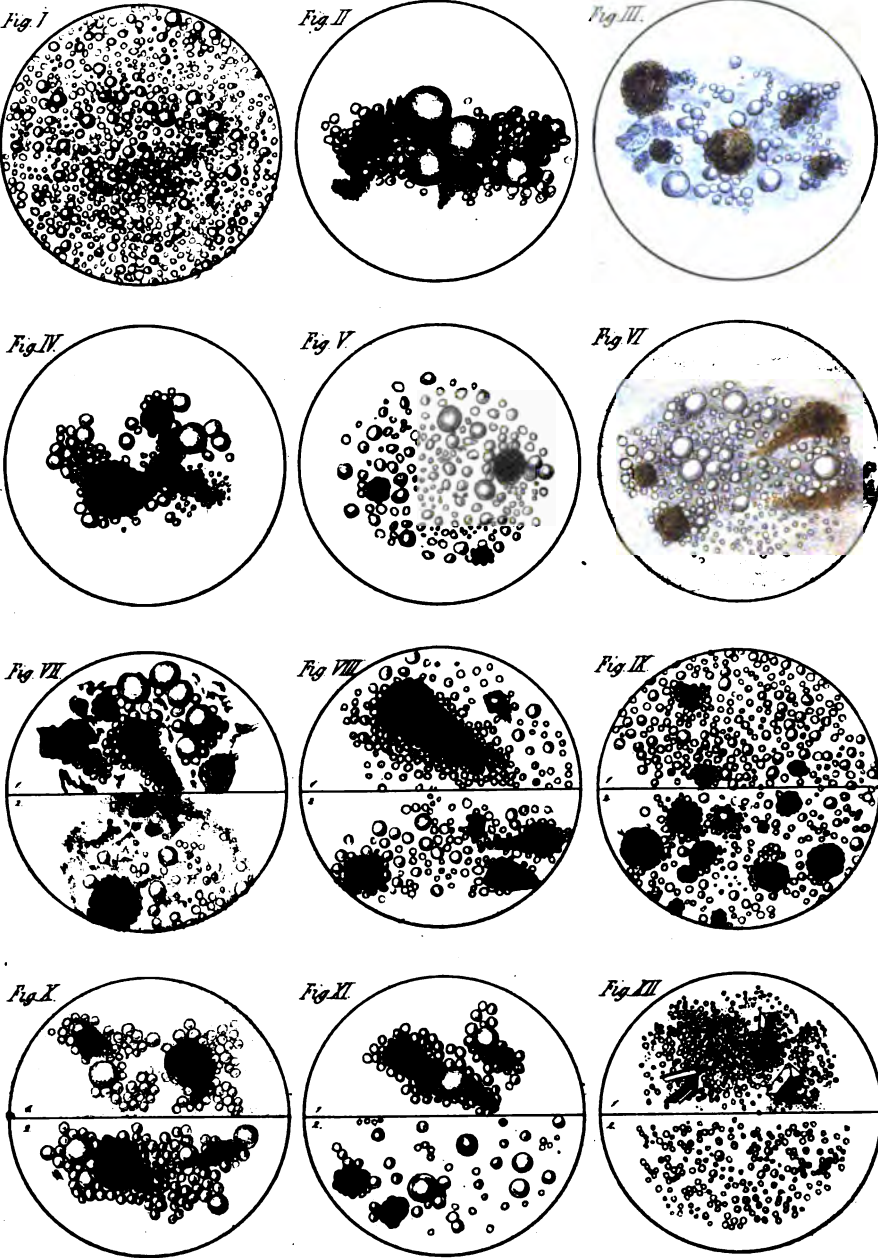
1. *The British and Foreign Medico-Chirurgical Review*. Received regularly.
2. *The Dublin Quarterly Journal of Medical Science*. Received regularly.
3. *The Provincial Medical and Surgical Journal*. Received regularly.
4. *The London Medical Gazette*. Generally received about four days after publication.
5. *The Medical Times*. Received regularly.
6. *The Dublin Medical Press*. Received regularly.
7. *The Chemical Gazette*. Generally received a month after publication.
8. *The British American Journal*. Received regularly.
9. *The American Journal of the Medical Sciences*. Received regularly.
10. *The Medical Examiner*. Received very irregularly.
11. *The Charleston Medical Journal and Review*. Last No. received for March 1848.
12. *Archives Generales de Medicine*. Received very irregularly.
13. *Annales de Therapeutique*. Received irregularly—the last No. received is for Avril 1848.
14. *Annales Medico-Psychologiques*. Received very irregularly.
15. *Journal de Medicine et de Chirurgie Pratiques*. Received regularly.
16. *Bulletin General de Therapeutique*. Exchange accepted—Please send by Mr J. B. Bailliere's parcel.
17. *Gazette Medicale de Paris*. Received regularly.
18. *L'Union Medicale*. Received regularly.
19. *Gazette des Hopitaux*. Received regularly.
20. *Bulletin de l'Academie Royale de Medicine de Belgique*. Received very irregularly.
21. *Gazette de Strasbourg*. Received very irregularly.
22. *Prager Vierteljahrsschrift fur die Praktische Heilkunde*. Last No. received is No. IV. for 1847!
23. *Zeitschrift fur Rationelle Medizin*. All the Nos. have been received, including No. I. for 1848.
24. *Zeitschrift der K. K. Gesellschaft der Artze zu Wien*. Last No. received is for September 1846!
25. *Medicinische Jahrbucher, and Oesterrichische Wochenschrift*. Last Nos. received are for June 1847!
26. *Archives fur Physiologische und Pathologische Chemie und Microscopie*. Last No. received, No. V. 1847!
27. *Gazetta Medica Lombarda*. Received regularly.

As our numbers are forwarded with the greatest regularity to all these Journals every month on the day of publication, we have to request the Editors of these Journals (especially those from Austria) will make some efforts to send us the exchange copies as soon as possible.

NOTICES TO CORRESPONDENTS.

Owing to the press of medical news and other matter at a late period of the month, we have been obliged to leave out some reviews and bibliographical notices which were intended for insertion in this Number.

Mr LYON's communication has been received.



A. Peddie del.

F. Schenck lithogr. Estlin.

ILLUSTRATIONS OF DR PEDDIE'S PAPER ON THE MAMMARY SECRETION.
FIGURES MAGNIFIED 206 DIAMETERS. (To face page 65)

THE
MONTHLY JOURNAL
OF
MEDICAL SCIENCE.

No. XCII.

AUGUST, 1848.

No. 26. NEW SERIES.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Mammary Secretion; its Character, Chemical and Structural; the Value of the Microscope in the study of it; its Existence an important Sign of Early Pregnancy; and its Pathological Changes; with Hints regarding Lactation and the Choice of Nurses.* By ALEXANDER PEDDIE, M.D., F.R.C.P., Physician to Minto House Hospital and Dispensary, Edinburgh.

THE chief design of this paper is to show, that the existence of milk in the breasts is an important sign or evidence of pregnancy, more especially of a first gestation, and in the earlier months; and to enforce attention to the practical value of the microscope, with the view of detecting the pathological changes of milk, and discerning cases where the propriety or impropriety of lactation, on account of the health of mother or child, may be under consideration. To render my remarks, however, more intelligible to those who may not have given particular attention to the subject of the lacteal secretion, it seems necessary that I should make a few preliminary statements regarding the function of the mammary glands, and the character, chemical and structural, of the fluid formed by them; and, to make this communication more complete and generally useful, I shall in its latter part introduce some general remarks on nursing, the choice of nurses, and the evils of unduly prolonged lactation.

I.—*The Functions of the Mammary Glands, and the Characters of the Lacteal Secretion in its Normal State.*

Providence, kind and wise in every arrangement, has in all respects perfectly adapted these glands, with their peculiar form and

NEW SERIES.—NO. XXVI. AUGUST 1848. 1

distribution of tubes, blood-vessels, absorbents, glandules, follicles, and epithelial cells, to the purpose of elaborating from the blood a fluid admirably suited for animal nourishment; and while the apparatus and its products are thus so well fitted for supplying physical wants, connected with them are generated sympathies, organic and emotional, which likewise assist in the conservation and growth, first of the embryo, and then of the infant, and draw forth those keen and strong affections which contribute to the mother's health and happiness.

To the accomplishment of those ends, as womanhood is approached the mammary glands are gradually developed, and feelings and desires are excited, and uterine sympathies established, which prepare them for the exercise of their destined functions. Whenever, therefore, conception occurs, these organs and sympathies receive an impulse to activity; so that, on the arrival of the period when the child must maintain a separate existence, the mother is enabled to furnish a supply of food of all others best fitted in quantity and quality for its sustenance.

Human milk, when perfectly healthy, is, to external appearance, thin, turbid, and white, with a slightly bluish tinge. It has a sweet taste, acquires a cream when allowed to settle, will afford butter if churned, will precipitate curd from the serum or whey if mixed with the mineral, acetic, or lactic acids, and finally cheese, if this curd is submitted to pressure. The oily part forms the cream and the butter,—the former by the ascension to the surface of the larger globules, and the latter by the union or incorporation of these with each other; while the casein or albumen—for they appear, according to Liebig, to be identical¹—gives the cheese. The specific gravity is from 1·030 to 1·035. It is decidedly alkaline when fresh, but becomes acid when kept for any time. Its chemical analysis has been stated by Simon to be

88·06	water.
3·70	casein.
4·54	sugar.
3·40	butter.
0·30	saline matters, &c.

100·00.

Its similarity, therefore, to the constitution of the blood itself is evident;² and reflection on the combination of the saccharine with the oleaginous and albuminous elements, will render apparent its life-sustaining and nutrient properties.

From the above analysis it will also be seen, that the fluid or

¹ Mulder has demonstrated that casein and albumen have the same bases, namely, protein.

² The chemical relations of blood and milk are exceedingly well stated by Dr Rees in the *Cyclopædia of Anatomy and Physiology*, Article *Milk*, p. 363.

serous portion of the milk is much larger than the solid. This part holds in solution the sugar, various saline ingredients, and the largest amount of the caseous element; while the oleaginous or buttery globules, with their thin, caseous pellicles, are suspended and float in it. Under the microscope the milk is seen to consist of a serous fluid in which innumerable globules are suspended. These, when viewed immediately, are seen to have active molecular movements, rolling past or over one another in their own independent courses. They vary in size, according to Donné,¹ from 1-500th to 1-50th of a millimètre. This admeasurement is differently stated by various observers, and the medium size is given as 1-4000th or 1-4500th of an inch in diameter;² but I should suppose that definiteness must be next to impossible. They appear—as Fig. I. of the annexed plate, drawn with the aid of a fine achromatic microscope magnifying 206 diameters will show—to range from minute molecular spots to pretty large globules. The smallest seem to be wholly or mostly caseous, while the largest are those described as cream globules. They are perfectly spherical and pearl-like, have clear dark margins, and bright, light-refracting centres. The surrounding margin has a distinct film or pellicle, which, however, the best observers do not recognise as a true organized cell, such as the envelope of the pus or blood corpuscle, but simply as a coating of caseous or albuminous matter, as pointed out by Mandl;³ and Dr Ascherson of Berlin⁴ has shown, that a drop of oil and a drop of the white of egg, dexterously mixed, produces an appearance identical with that of the milk globule. Ether dissolves the oil of the globules.

These, then, are the characters of the lacteal secretion, as found in its normal condition when the mother has quite recovered from child-bed, and is suckling her child. But until the process of secretion is fairly established, the milk is marked by other features which it is of importance to notice, namely, the presence of what is called colostrum. The colostric fluid—popularly termed “green milk”—serves an important purpose in the infant economy, being nature’s aperient for purging off the meconium which has accumulated in the large intestines during foetal existence. When present in great quantity, it gives to the milk, seen by the naked eye, a yellowish colour; and, on examination under the microscope, it is found to differ most essentially from the secretion already described. In examining a single drop two or three days after parturition, within the field of the microscope, while numerous proper milk globules may be seen moving independently here and there, they are agglomerated into masses, adhering together by a viscid mucoid fluid; and at the same time there will be seen from one to four, or even more, colostric bodies. These,

¹ Cours de Microscopie.

² Hassall’s Microscopic Anatomy of the Human Body.

³ Anat. Microscopie, p. 53.

⁴ As quoted by Dr Bennett, in his paper on the Structural Relations of Oil and Albumen.—*Monthly Journal of Medical Science* for 1847, p. 167.

described by Donné¹ as the "*corps granuleux*," vary much in form and size,—a few resembling large oily globules, evidently better elaborated, forming a yellow bed, in which lie a multitude of dusty looking granules, and among which is generally imprisoned one or more true milk globules. The envelope to these masses is at first firm and transparent, but breaks down as the secretion becomes older, and the globules become individually more independent of each other.²

I have given a representation of these appearances in Fig. VI., which will answer the purpose of a lengthened demonstration; and as this specimen was taken from a patient in the best of health three days after delivery, and promising to become a most excellent nurse, the same delineation will show the various stages towards the disappearance of these bodies, namely, the breaking down of the firm envelope or binding membrane, the shedding of its contents, and the last cloudy streak or patch which is visible. These changes go on in general until about the tenth day after delivery, though certainly in many good nurses for about three weeks; and then the milk ought to possess the normal characters (Fig. I.) already described. The granular portion of these masses is soluble in ether; the membranous portion in acetic acid; and treated with ammonia the mass becomes glassy and tenacious.

Here I may notice, although having no practical bearing upon the present inquiry, that I have examined microscopically many specimens of the milk which is well known to exist in the breasts of young infants. I have found it almost invariably present, seldom obtainable until the second day after birth, generally most abundant about the fifth day, and usually gone by the tenth day. It presents all the characters of the mother's milk at the same period, with colostric bodies as well as milk globules,—only the latter are in sparing

¹ P. 400.

² There is an evident similarity between colostric and compound granular bodies,—those formations which Gluge denominates *inflammation globules*, which Vogel calls *granular cells*, Henle, *exudation corpuscles*, and Bennett, *exudation masses*. These structures are seen in cerebral softenings, and inflammatory products of various organs; but M. Reinhardt has shown that they are not necessarily characteristic of inflammatory action. In the digest of his opinions given in the *Monthly Retrospect* for Feb. 1848, published along with the *Monthly Journal*, it appears that M. Reinhardt considers the colostric granules as transformations of the epithelial cells of the mammary ducts—as the result of disintegration, or a retrogressive metamorphosis. During pregnancy, when there is an increased action going on in all the tissues of the mammary glands, and a greater determination of blood to them, the epithelial cells are thrown off in greater numbers than usual, distending the milk tubes. "This superfluous epithelium, therefore," the editor says, "is converted into granular cells or colostric corpuscles, and thence into masses of agglomerated fatty granules, which are then broken up and absorbed. During the first days after delivery, these bodies continue to be developed, and are washed out with the first formations of milk which constitute the colostrum." The colostrum generally exists in greatest quantity after first deliveries.

number, and generally of larger size. I find also that it is as often present in male as in female children, and may be got most readily by a gentle pinching or pressure on each side of the tiny nipple. I have given representations of this infantile milk in Fig. XI. Nos. 1 and 2.

After these details, I now go on to consider the existence of the mammary secretion *during* gravidity; and to enforce the practical importance of a knowledge of this fact in the diagnosis of that condition.

II.—*The Mammary Secretion prior to Parturition; and its Existence a Sign of Pregnancy.*

In common with others in the profession, when considering the differential diagnosis of obstructed menstruation and pregnancy in the earlier months, with all due attention to the ordinarily recognised signs, I have often experienced serious difficulty as to the opinion to be given, or the treatment to be pursued; and I believe that I have been misled in some instances by those whose object was, if possible, deception and concealment. The dilemma is most apt to occur in the discharge of hospital and dispensary duties, when required to give advice to young females whose characters are unknown. Under all circumstances, the responsibility which rests upon the physician is great. To convict an applicant of an immoral act, and of present imposture, and avoid the risk of prescribing means which might blight a human existence; or to allay a mother's fears of a daughter's virtue, and silence the tattle and cruel suspicions of a neighbourhood, are assuredly important objects. The determination may concern the health, happiness, and prosperity of individuals and families, and greatly affect the practitioner's peace of mind and prospects in life. Indeed, in every point of view, as well as from regard to moral and medico-legal ends, it is an imperative duty in any doubtful case, to submit to a searching scrutiny every sign, symptom, and circumstance which can throw light upon it.

From the experience of upwards of two years, during which my attention has been directed to the subject, I feel convinced that *the most invariable sign of gestation prior to quickening, is to be found in the presence of fluid in the breasts*,—with the limitations that shall hereafter be noticed; and, consequently, that the absence of the secretion will afford the surest evidence that the suspension of the catamenial flux is an abnormal deviation from nature's course. The sign is indeed, to some extent, an old and popular one, but not to be despised or overlooked on that account, nor to be set aside without consideration. Although some eminent writers on obstetric medicine have passed it by without any notice, and others have pronounced it "an evidence scarcely of any value at all,"¹ I would earnestly invite an unprejudiced attention and scrutiny of the subject; for my own experience

¹ Churchill, *Theory and Practice of Midwifery*, p. 107.

in judging from the sign, when there was an opportunity of watching the course of events, warrants in me stating, that I have never found it fail in regard to those *who were gravid for the FIRST time*, or in regard to those who were not pregnant at all. And although the greater number of cases of pregnancy in which I have had an opportunity of applying the test, have been advancing in the fourth month, yet so early as the end of the second and the beginning of the third months the sign has held good. It is not, however, until about the termination of the third, and more generally in the currency of the fourth months, that the medical man is consulted, when the repeated non-appearance of the menses attract the notice, and excite the interest or fear of the individual, according as the moral feelings may be affected. And as the value of the sign is not insisted upon in its application to other than first pregnancies, within this limitation is included the class of cases, which, above all others, are of most frequent occurrence and importance, and which occasion most trouble and anxiety to the practitioner.

Perhaps the value of this sign in the early months, has been doubted or under-estimated, in consequence of not observing fluid trickle from the nipples, as it frequently does in the last month of pregnancy. I believe that this will rarely happen in the commencement of a first gestation. The fluid must be brought; and the method of obtaining it, under doubtful circumstances, is to press the finger and the thumb firmly on the mammary gland, a little beyond the margin of the areola, and then draw them to the point of the nipple with a stripping and expressing movement. This repeated three or four times, will certainly bring fluid if any be present; and a single drop will suffice to prove the nature of the case. A little moisture from the sebaceous follicles of the areola, which is sometimes produced during these efforts, must not be mistaken for a lactic secretion. I believe that this kind of exudation, which may occur in ordinary circumstances, has actually led, in some instances, to the rejection of the sign which I am now advocating. It is recommended also, that if a drop of fluid is not obtained from one nipple, the other ought to be tried, as the orifices of the lactiferous tubes are sometimes more narrow or glued up in the one than in the other breast, and in primiparæ this is more particularly the case.

In the early months of a first pregnancy the secretion has seldom the external appearance of milk, but is serous-looking, and often very viscid and syrupy. When submitted, however, to the microscope, the characteristic milk globules (described p. 67) will at once be detected; and these will be seen agglomerated *en masse*, the solid portion being at this period in a large ratio to the fluid, which latter is also peculiarly glutinous. Mixed with these groups will be perceived an abundance of large oil globules and colostrum granules, as in the green milk of recent parturition. There are sometimes found also a few epithelial lamellæ, which have been separated from the lining

membrane of the excretory ducts, and which have either not been transformed into colostric masses, or, if this has been so, they have already parted with their mucoid and granular contents.

Of the annexed plates, II. and III. are exact copies of the secretion taken in the third and fourth months of gestation; the first, from a young unmarried woman, who attempted to conceal her pregnancy; and the second, from a young unmarried lady, who was not aware of her own condition, and whose station in life, education, and previous good conduct, was a protection against an early suspicion of her state. Both were convicted by the milk test, when the ordinary signs excited only a vague suspicion; both soon confessed their transgression; and both were, on the strength of the opinion given, immediately placed in the bonds of lawful wedlock.

The value of the lactic secretion as a sign of pregnancy has apparently been disregarded by some, in consequence of the very exceptional cases recorded, more especially the example given by Baudeloque, of a girl eight years old who was able at pleasure to milk her own breasts, and another somewhat similar mentioned by Belloc; and this under-valuation may also be ascribed to the statements of other writers regarding the exudation of milk, even from the breasts of adult males.¹ Such cases, however, even if well authenticated, are worthy only of being ranked among other physical monstrosities occasionally met with; and even instances less wonderful, as the appearance of fluid in the breasts of those who are not, and never were pregnant, ought, I think, to be viewed as rare examples of nature's freaks—her exceptions, and not her rule.² While I do not doubt that such cases have occurred—though I think some of them are not unlikely to have been the follicular exudations already noticed—in the very numerous examinations which I have made with a view to detect, if possible, the existence of fluid in the mammæ of the non-pregnant, I have not as yet met with an instance of the kind. In many instances of unmarried women of unblemished character, who were suffering from menstrual obstructions, and of married women under similar circumstances, who never were gravid, I have not been able by expression to obtain a single drop of fluid; and in many instances also of both classes where there was perfect uterine health, I have been equally unsuccessful.

Beyond the limitation of first pregnancies, I would not desire to

¹ Carpenter in his *Treatise on Physiology*, p. 626, refers to an instance of this kind, as described in the *Phil. Trans.*, vol. xli. p. 813; another by Captain Franklin, in his *Narrative of a Journey to the Polar Sea*, p. 157; one by Humboldt, in his *Personal Narrative*, vol. iii. p. 58; and a fourth by Dr Dunglison, in his *Physiology*, vol. ii. p. 417. In the last-mentioned case, the subject, a man of colour, is said to have actually officiated as a wet nurse!

² In this light I would regard the single instance which M. Donné has noticed, of a little fluid found by him in the breast of a young woman said never to have been pregnant, and which presented the microscopic character of milk, p. 441.

urge the application of the milk test; for when a woman has once suckled, the fluid is apt to linger in the breasts a considerable time after weaning, and the mammæ continue performing a partial function,—in many instances, doubtless, owing to the daily operation of maternal sympathies. In general, I have found the fluid easily obtainable from three to six months after weaning, and the restoration of the catamenia,—although in one instance which occurred lately, it had completely vanished before the expiration of two months. On the other hand, I found it present in one case after the lapse of two years; and I believe that it may exist at a still later period, although in several women still within a child-bearing age, I have been unable to procure a single drop of fluid at the distance of four years from the date of last weaning. It may, however, readily return at a much later period in the case of those who have once given suck, when the uterus becomes distended with any false conception or hydatid accumulation; and very probably it may do so under similar circumstances, in those who never were pregnant. An instance of the former kind came lately under my own observation. The patient had not had a child for nine years, and while the uterus gradually enlarged, and the abdomen became distended, so as to simulate pregnancy, and lead to the necessary preparations, imposing on myself at an early, and on another physician at a later period, the secretion was most abundant. The time of expected confinement, however, passed by to the extent of several weeks, and the mystery was solved by a sudden and large discharge of water.

When milk is procurable a few months subsequent to weaning, and the woman not again gravid, I have found it existing only in small quantity, one or two drops, viscous and cream-like to the unaided eye; and to the microscope it presents milk globules in sparing number, often ill formed, adhering together, or to large oily drops (the creamy part), or to the epithelial scales, mucoid, and other foreign matters, which are generally present in abundance—as is seen in Fig. VII. 1 and 2. The secretion in such circumstances affords, I think, some good diagnostic marks, by which it may be readily distinguishable from fluid obtained in the early months of a new gestation. The latter is comparatively rich in milk globules of normal appearance, and of better medium size, having abundant colostrum, and few if any membranous scales, or debris of disintegrated textures—(Figs. II. and III.); and, as the full term of gestation is approached, the secretion increases in quantity, and becomes better in quality, more evidently suited for the important object in prospect.—(Figs. IV. and V.)

From the above notices, I think it may be admitted that the sign of pregnancy now advanced, although of most value in the diagnosis of a first pregnancy, is not without a certain amount of importance in the recognition even of a subsequent gestation; and I should decidedly affirm, that in general it is more certain in its information as a corroborative evidence of pregnancy than the papular

areolæ, which, when once darkened, seldom lose much of their colour, or of the follicular glands, which retain in a great measure their size after they are once developed.

Compared with any of the ordinarily recognised signs for distinguishing a first pregnancy from a simple suppression of the menses, before any bulk or impaction in the iliac and hypogastric regions can be detected by the eye or hand, or before the ear can discover the unmistakable sounds of placental and fetal circulation, there will be found, I think, far fewer exceptions to the milk test. As regards the sign of *morning sickness*, I have had under advice many cases of obstructed menstruation from causes unconnected with gravidity, yet attended with disturbance of the digestive organs to a great extent, of which daily recurring sickness—most frequently in the morning—formed a part; while, on the other hand, in very frequent instances this symptom has been entirely wanting in those really pregnant. Then, again, the signs taken from the *aspect of the mammae*, are most variable and contradictory. While I have often observed women with fair complexions who had large breasts, well marked areolæ, numerous and large follicular glands, and prominent nipples, suffering merely from suppression of the catamenia, I have seen not a few with dark or sallow complexions, who, although undoubtedly pregnant, had small breasts, small nipples, areolæ scarcely distinguishable from the surrounding skin, and few or no sebaceous glands.¹ Were it not to extend this communication to too great a length, I might give notes of many cases illustrating the variability and uncertainty of these signs. I shall, however, content myself with noticing only one example—which is interesting in several other respects. The patient was brought before the Obstetrical Society in December last² by Dr Simpson, chiefly to show the impossibility of diagnosing pregnancy in her case, in so far as the appearance of the *mammæ* were concerned. The woman had dark brown hair and a sallowish complexion; she had been four years married, and was then, although presenting no traces of areolæ or glandular follicles, decidedly in the seventh month of her first pregnancy, complicated with large fibrous tumours projecting from the anterior wall of the uterus. Now, it is an interesting fact, that before the fourth month was complete, counting from the last menstrual period, I had carefully examined this patient, who called on me on account of the tumour

¹ In so far as the appearances of the breasts are concerned, it may be curious to notice here, that I had under my care lately an adult male of fair and ruddy complexion, afflicted with disease of the heart, whose breasts were full, slightly pendulous, surrounded with exceedingly dark areolæ, numerous follicles, and whose nipples were large and prominent. On several previous occasions I have met with cases somewhat similar; and in fact the variety in the colour of the male areola, the development of its follicles, and the size of the nipple, is a matter of daily observation.

² Monthly Journal of Medical Science, March 1848, p. 693.

of the abdomen; and, in consequence of being able to extract a little fluid from the nipples, I expressed my conviction that she was pregnant, though I was then at a loss to say whether the tumour was a growth from the uterus or an extra-uterine conception. On a second examination, about one month subsequently, I was able by the stethoscope to verify the opinion formerly given as to the fact of gravidity, and I know that she has been since confined at the proper term of gestation.

As regards the evidences of pregnancy from *abdominal exploration* previous to the commencement of the fifth month, there is always ground for much doubt and fallacy. For it is not till then that quickening occurs; and in cases requiring a special scrutiny, this is a symptom which will in all probability be concealed. It is not until then also, that the iliac and hypogastric regions assume a visible fullness, and give to touch a feeling of firmness and impaction; and, although this enlargement was then apparent, it might be owing to causes very different from gravidity. Auscultation, too, comes after this period only to be of any avail,—and perhaps not even until a considerable time subsequently, to the ears of many; and did delicacy oppose no obstacle to uterine examination, until now the finger or the speculum of the most experienced obstetrician, may be unable to detect the true nature of the case.

The presence in the urine of what has been called—but absurdly so—*Kiestein*, is a very important evidence of the existence of pregnancy. It has been found by Dr Golding Bird¹ as early as the second and third months after conception; but as it appears to exist only in small quantities in the early months, as the urine must be allowed to settle for a number of days before the peculiar greasy, cheese-odoured pellicle can be obtained, it is evident that this is not a test which can be often or conveniently employed, especially in the most important class of cases in which the physician is consulted. The fact of the unvarying existence of this substance, however, may I think be assumed as strongly corroborative of the value of the milk test, as it shows the presence of the secretion at an early period; for without doubt, as Dr Bird expresses it, “the imperfectly formed secretion of milk, not having a ready exit by the *mammæ*, is taken up into the circulating mass, is separated by the kidneys, and eventually escapes from the body in the urine.”² With the aid of the microscope, I have fully satisfied myself that this product contains some of the elements of milk—the largest amount of which is probably caseous matter, mixed with crystals of the triple phosphate of magnesia—(Fig. XII. No. 1.) That the milk should be thus changed in appearance—if the theory given above is correct—is not surprising, on account of the process which it must go through before its elimination can take place. As a contrast to this, I have given a representation also of the change produced on milk after absorp-

¹ Guy's Hospital Reports, Vol. V. p. 16 and 25.

² Ibid. p. 22.

tion from the alimentary canal, and excretion by the urinary apparatus—(Fig. XII. No. 2.) It is the greasy pellicle of an infant's urine allowed to settle for fifty-eight hours, and observed by the microscope. The child was deriving its sole nourishment from the breast, and was in the possession of perfect health. A few pretty well-formed globules are apparent, but most of them have lost the rotundity of the original milk globule—are flatter, and present a much thinner investing membrane. One ounce of the fluid was sufficient to yield a well-marked scum; no crystalline matter was present. The delineation of the kiestein which I have given, closely resembles the aspect of caseine when precipitated from the serum of milk by the action of acetic acid,—as is depicted in Hassall's *Microscopic Anatomy of the Human Body*, Plate XV. Fig. 5.

III.—*The Pathological Changes of Milk; the Value of the Microscope in the Observation of such; and Hints regarding Lactation and the Choice of Nurses.*

I now proceed to remark on the deviations from the normal condition of the milk—in other words, *its Pathological Changes*—which are detectable by the use of the microscope only; and to point out the importance of a knowledge of these, and the circumstances connected with them in practice. For many interesting and instructive remarks on this branch of the subject, I would again refer to the excellent treatise of M. Donné. Having made the greater part of my observations before becoming acquainted with his researches—while much trouble was unnecessarily occasioned, I have had the satisfaction of verifying all that he advances; and if I am not able to bring forward much that is new regarding the microscopical aspects of the mammary secretion in health and disease, I may perhaps succeed in drawing more attention to a subject which has been much neglected, and placing in a clearer light facts which appear to be little understood, and which are high and wide in practical import.

M. Donné makes no allusion to the existence of the mammary secretion previous to the last month of pregnancy, and consequently has given no account of its microscopic characters at an earlier date. This defect I have been able to supply, as will be seen in the description given at p. 72. The Figures II. and III. show the peculiarities during the third and fourth months, and IV. and V. during the seventh and ninth months of gravidity. Drawings illustrative of appearances at intervening periods might easily have been multiplied from the numerous observations made; but those given will suffice to show the chief phases of the secretion during the progress of gestation; and that, while in the earlier months the elements of good milk are apparent as the term of pregnancy approaches towards completion, there is a manifest improvement in the quantity and quality secreted—gradually becoming better adapted for the purpose for which it is so admirably designed. It may

especially be remarked, that the foreign matters, such as the epithelial scales, are less likely to be met with; that there are fewer large butyraceous globules present; and that, from the lessened viscosity of the serous part of the secretion, there is a smaller tendency to aggregation among the milk globules. The colostric bodies, which are present from the very first, continue so, as I have already stated at p. 68, until perfect convalescence from child-bed. It sometimes happens, however, that the milk is not so soon, or is sometimes never, freed from its colostric characters in one form or another, or that these return again at some period during lactation, after the secretion has been perfectly pure. When this occurs, as M. Donné has had the merit of showing, the health of the mother in affording such unwholesome supplies, and the child in receiving them, are placed in equal jeopardy. Of the truth of this opinion, I have found ample proof. M. Donné goes the length of expressing his belief, that from microscopic examination of the secretion alone in the last month of pregnancy, we may ascertain whether a woman is likely to prove a good nurse or not.¹ This I consider to be extremely probable; but be it so or not, there is no doubt that, while certain information cannot possibly be obtained from the external characters of milk during suckling, such as colour, consistence, and taste, or by the application of chemical tests, the microscope enables us at once to detect deviations from its normal condition. Assuming, therefore, the appearances described at p. 67, and depicted in Fig. I., as the standard purity of the secretion, it is surely rational to employ this means—while other considerations are not neglected—in determining those momentous questions which so often occur in regard to the propriety of a mother suckling her own offspring, as the strong ties of nature and the suggestions of duty would dictate, the length of time lactation may be continued with safety to herself and benefit to her child, or the choice of a nurse as a substitute, by whom the future health and happiness of the infant may be so materially affected.

During lactation, the nurse cannot be indisposed without an immediate change occurring in the properties of the milk; and to such vitiation is evidently attributable many of the alarming illnesses of infants. Every nursing mother knows, how apt a purge taken by herself is to affect the child, while she obtains perhaps little benefit from its use; and how speedily some articles of diet excite flatulent distension and colic pains in the infant. It is also a well-known fact, that when a nurse is caught in a shower and gets damp, or is exposed to any sudden danger, or hears any painful intelligence, and becomes violently agitated,—the child is apt to be rapidly and forcibly affected. This latter cause has been known to produce convulsions, and even death.² Hence, it is the duty of those who have the charge of young and inexperienced

¹ P. 406.

² Eberle on Diseases of Children, p. 35.

mothers, to give wholesome advice regarding the diet and regimen, the care of the breasts, the subjugation of the desires and passions, the cultivation of equanimity of temper and cheerfulness, and the avoidance of unsuitable medicinal agents; and hence, also, the need of keeping a strict look-out on the condition and conduct of the hireling nurse, who has not such strong natural incitements to caution, or motives to self-denial. Now as it is evident that it is through the channel of the breast that many disorders are conveyed to the child, it is equally clear that, in the elaboration from the blood of the milk in such instances, some alteration must take place in its nature, whether perceptible to our senses or not, imparting to it an injurious tendency. As it is in the atmosphere, however, which we breathe, and which may convey to us some severe or pestilential disease, and yet the utmost efforts of science for detection and analysis may be baffled; so perhaps the most skilful histologist, with the best optical instruments, and the most patient attention, may never be able to discover minute deviations from the normal state of the mammary secretion corresponding to the various diseased conditions of the mother, or the disorders produced in the child. Thus, there has nothing hitherto been ascertained peculiar or specific in the milk of a syphilitic nurse, although the virulence of the taint communicated is obvious enough; thus, too, nothing distinctive has been observed in the secretion of the nurse who may be communicating strumous disease to the child of another individual free from all hereditary taint, which in after life exhibits itself in one or other of the dreadful forms of diseased glands and joints, phthisis, or insanity. Even in the secretion from the breast of a patient extensively affected with cancer, which I lately had an opportunity of examining,¹ I could detect nothing essentially different from the appearances of fluid taken from an engorged breast, or in the cases hereafter to be mentioned. The only pathological changes of the milk which have as yet been observed, are the colostric condition already described, and admixture with pus. Associated with the more severe ailments of the mother, which seriously affect the child if not withdrawn from the breast, I have observed that the granular bodies are larger or more numerous, and more frequently of a rich amber colour than of the yellowish or greyish tints observed prior to delivery, and during child-bed. There seems also to be a much greater tendency to aggregation of the real milk globules from the amount of mucoid fluid present, which is turned into a most tenacious mass with ammonia; and there is often seen on the field of the microscope thick patches of grey, yellow, or deep amber-coloured matter, floating raft-like, with milk globules resting on

¹ The tumour in the breast of this individual was first observed about three months previously, and had since grown with great rapidity, involving a large portion of skin, and implicating a gland in the axilla. The child, now sixteen months old, although only partially fed from the breasts, was sickly-looking, and troubled with bowel complaint.

them or clinging to their edges. These characteristics vary in degree in relative correspondence to the deranged health of the nurse and nursling.

In instances of returning catamenia,¹ a single day is sufficient to occasion a considerable change in the milk; and in this affection, as is well known, the infant often immediately refuses the breast, and if not so, vomits severely, and is pained or purged. This abnormal change is seen to a still greater extent in cases of fever, of which Fig. VIII. No. 1, is an example. The patient from whom this fluid was taken, was in the fifth day of the fever. She had made an excellent nurse during two months, but had not suckled for the last two days. There was no engorgement of the breasts, but, on the contrary, they were pendulous and almost empty. The infant in this case did not suffer, as it was withdrawn early from the breast, and suitable nourishment substituted; and as the fever was of short duration, and not very severe, the secretion was kept up by a little being artificially drawn off from time to time, until the child could be reapplied with safety. The same appearances of the milk are observed in cases of severe dyspepsia and general debility, such as is depicted in Fig. VIII. No. 2. This patient nursed nearly eight months, during the two last of which she had gradually been losing strength and flesh, and complained of violent headaches, vertigo, disrelish for food, and other symptoms of indigestion. The child during the same period was much affected, although perhaps dentition may have had a share in its ailments. It was pale, soft, fretful, and had occasional attacks of violent abdominal pain, vomiting, and diarrhoea. Weaning was in this instance recommended to save both mother and child from certain ruin; and the step was followed by the best consequences, more especially to the health of the child.²

Many similar cases have occurred to me, which it is unnecessary to particularize,—cases happening at various stages of nursing, some of which required its immediate and entire abandonment, some a temporary cessation from it until the general health was restored to its normal condition, and others where only a partial performance of the function was permitted. The treatment applicable to those various cases, such as the choice of a substitute nurse, the kind of

¹ As yet no blood globules have been detected in such cases; and it appears that blood is never secreted from the lactiferous tubes during suckling. When blood is drawn, it is from rupture of a capillary vessel by too strong suction.

² It appears that in cow's milk granular bodies and agglomerations of globules are present for some time after calving, and that when the animal falls into bad health there is a reappearance of the same characters; and M. Donné has shown that in the disease called *la cocote*, characterised by fever, pustules in the mouth, udder, and hoof—a malady apparently analagous to our "murrain,"—the milk exhibits the same abnormal aspect. Hence the value of a microscopical examination of an article so important in domestic economy, when it is suspected to be rendered impure by the disease of cattle. And great use may also be made at times of optical help and chemical reagents in detecting the adulterations of milk by various substances.

supplementary food proper at different ages of infancy, &c., shall be briefly noticed at subsequent parts of this paper, in so far as is consistent with its special object.

When nurses are affected with returns of the catamenia, I do not think it necessary to put a complete stop to suckling, unless they recur frequently, and are evidently impairing the mother's health. For the security of the child, it is enough to withdraw it from the breast during the continuance of the flux, and otherwise supply suitable nourishment, while the secretion is drawn off several times a-day at regular intervals, either by the lips of an attendant or a suction apparatus:—the first of which modes is preferable, if done slowly and tenderly. I have followed the same practice in other short illnesses of the nurse, occurring prior to the proper time for weaning, until the constitutional disturbance subsides. An engorgement is thus avoided which would produce a bad effect on the glands, and prove certainly hurtful to the child when suckling is resumed. This is known to happen in simple engorgement even of one breast, while the other is performing its proper function, for the milk in it becomes viscid and colostrous. It is of obvious consequence, therefore, to impress upon the inexperienced, from the moment of delivery, the necessity of managing the breasts so as to avoid any cause which might favour over-retention of milk in the mammae, or a determination of blood to the organs, and thus occasion engorgement, febrile excitement, and perhaps even inflammation and suppuration. It is always expedient during lactation to have the breasts regularly and alternately drawn at intervals of from one hour and a half to five or six hours, according to the age of the child, generally oftener in the case of young children, and less seldom by those who are further advanced, and who are the length of obtaining other kinds of nourishment; and when an engorgement has been permitted to occur, the first drawing off should, if possible, be done otherwise than by the child. It is bad for the health both of mother and child, and materially affects the condition of the mammary gland and the quality of the milk, to deny the breast long after birth;¹ and it is equally bad practice at a subsequent period to give it too frequently,—whenever, in fact, the infant exhibits the slightest uneasiness or pain. To discuss such topics, however, would extend this communication to too great a length, and moreover they do not strictly belong to the subject of our inquiry.

Should an inflammation of the breast be established, the best practice is to remove the child from it at once, and permit it to drink only from the healthy one; while a little of the secretion, if pain will allow, is drawn off from time to time as tenderly as possible. For before suppuration has begun, in consequence of the

¹ The first trial ought to be made within a few hours after delivery, and repeated at intervals of from three to four hours, until the secretion is fairly established, which is generally in from three to five days.

congested and inflamed state of the glands, the quality of the milk becomes much changed, as already noticed; and after an abscess is formed, according to M. Donné, pus corpuscles may mix with the milk in the lactiferous ducts, and pass through their orifices in the nipples, consequently becoming unwholesome and unsafe for the child if still at that breast. That this morbid admixture always takes place I am not prepared to admit; for although I have often seen pus and milk mingling and flowing out at the openings of abscesses and sinuses, the milk from the nipple in these cases appeared, from its thin consistence and bluish-white colour, to be genuine and unpolluted. Microscopic examination, however, will of course be the certain means of detecting the presence of pus; and although in the only two cases I have had an opportunity of making use of the microscope—cases of extensive suppuration—I was unable to find a single pus corpuscle, I do not doubt the correctness of the statement made by so accurate an observer as M. Donné. I think it not unlikely, however, that the delineation given in Mr Hassall's work¹ has been small grey-coloured colostric bodies in tight envelopes, rather than pus corpuscles. I infer this from the large size of the latter compared with the former bodies, and from the granular appearance given to them. The appearances of some of the colostric granular bodies in No. 2 of Fig. IX., are illustrative of this conjecture; and the fluid depicted in this case was taken from a breast which had been suppurating and healing up alternately for three months, but had closed finally only ten days before, leaving the gland hard and knotted. The less regular margin, the spotted and opaque appearance of the pus corpuscles, compared with the clear border, spherical form, and translucent aspect of the real milk globules, ought to be good diagnostic marks in such cases. Should there, however, at any time be difficulty in deciding as to the admixture of pus with milk, the use of two chemical tests under the microscope will settle the point; for while real milk globules are readily soluble in ether, and are unaffected by a caustic alkali, the latter at once dissolves the pus corpuscles, leaving the milk globules untouched. Should any blood globules happen to be present, they may be disposed of by the addition of acetic acid, without interfering either with the pus or the milk.

In the milk of all women who nurse beyond what may be called the natural time, namely, nine months, I believe that the colostric characters will soon be assumed. Such has been the case in all the specimens I have examined microscopically. This, however, may happen some months earlier to women of a particular constitution, and in such instances lactation persisted in must be attended with bad consequences. Hence the utility of the microscope in enabling us to decide when the child should be taken away from the

¹ Microscopic Anatomy of the Human Body. Plate XV. Fig. 1.

breast, and to guard against the choice of a substitute who may not herself be better qualified to furnish a healthful supply.

The case already mentioned, accompanied with the delineation of the milk, Fig. VIII. No. 2, is an instance of early unfitness for nursing, and of the imminent danger from continuing it. Another example I have given in Fig. IX. No. 7. The patient from whom this specimen was taken had only nursed for six months. It was her first child; and about two months after delivery she was disabled from suckling with one breast in consequence of suppuration, although the abscess healed up in four weeks. She was much emaciated, dyspeptic, and presented on different parts of the body cutaneous cysts, known by the name of the *molluscum contagiosum*, of various sizes, from that of a pea to a large bean. Her child had the same affection to a considerable extent, eczema behind the ears, constant diarrhoea, and a weakly ill-nourished body. From the breast not suckled I could easily obtain a little fluid; and it presented characters identical with those in the case referred to at p. 80, and depicted in No. 2. of Fig. IX. The colostric bodies were seen to the number of from five to twenty in every drop placed in the field of the microscope; and these were also of small size, mostly ovoid in shape, and girt with a tight envelope. Were it requisite, I could quote many other cases in which microscopic examination satisfied me, that suckling at different periods prior to the termination of the ninth month could not be carried on without danger both to mother and child, or to either; and in several instances I have also been enabled to prove, when otherwise I could not have done so, that the illness of a child was in no way attributable to the milk of the mother. Thus the most tender and important objects and interests may be secured. The attention is at once directed to other causes of ill health; and the proper course of treatment is made more apparent. While in one instance violence is not done to the feelings of an affectionate parent, in depriving her unnecessarily of the pleasure and privilege bestowed upon her by nature, and by the transference of her little one to the breast of a stranger; in another instance, pain is not given to the hired nurse by an uncalled-for interdict on her future services. Thus both may be permitted to exercise a function which, from physical changes and mental sympathies, may prove most salutary to future health; and the child has continued to it the supplies of food best adapted to its tender age, and is not hazarded either by a change of milk, or by an artificial substitute, at a time when ill health makes the success of such an experiment more uncertain and unsafe. Thus too, by the help of the microscope, we may be able to distinguish between instances of real inability to nurse and of sheer disinclination to do so. In this age of luxury and refinement it is to be regretted that there are so many who belong to

the latter class,—individuals who seek to escape the trouble connected with the rearing of offspring, and the trammels which may interfere with a free indulgence in the pleasures and gaieties of the world.¹

Having now seen that the milk sometimes becomes impure, and consequently injurious, before the completion of the ninth month—the usual period for weaning, I go on to state the morbid changes which are apt to occur in it when that term is exceeded.

In a very few weeks after this period, and I should say almost invariably at the conclusion of one year from the birth of the child, the secretion is found colostric even in the healthiest women. To this I have met with no exception; and, seeing that such is the case, I believe that lactation continued beyond the ninth month is decidedly improper. When mother and child escape injury from such a practice, it is not in consequence of the ability to give, and the suitableness of the supply to the wants of the infant economy, but from the strength of the constitution of both to resist the evil effects which it tends to produce.

The impropriety of such prolonged lactation may appear evident to any one who reflects that the child is now being endowed with teeth, and its organism in all respects so far advanced in development as to require a much more nutritious fare than the mother can supply. Mothers, however, are often found continuing the child at the breast from personal considerations, without particular reference to its wants. What I allude to is, the supposition that during nursing protection is afforded against the occurrence of a new pregnancy. This idea I think is an erroneous one; and it is to be regretted that medical men have not set themselves more against the practice founded upon it. While it is admitted that those who do not nurse at all are apt to bear children in quicker succession than those who do; yet that those who delay weaning beyond nine months are less likely to conceive soon than those who do not, is opposed to all the experience I have had in practice. When fertile power is very strong, a new gestation will commence a few months after the last child-birth, even although the lactic function is in operation;—hence the frequency of pregnancy in the sixth month of suckling; when it is strong, conception is sure to take place about the termination of the ninth month of

¹ The best artificial nourishment is cows' milk, but diluted from one-third to one-half, slightly sweetened, according to the age of the child, as it is richer than human milk. The milk of the goat and sheep are nearly similar in strength, and that of the ass is lighter; but were the taste and smell of all these agreeable to every child, they are not generally and regularly procurable. The "*pan and spoon*" system is unquestionably pernicious to newly-born children; but when the fourth month is attained, then supplies of rusk, arrow-root, barley-meal, and other light farinaceous food, may be begun in small quantities, and varied as they are found to agree with the infant.

suckling; when it is ordinary, conception generally occurs about one year after the last delivery; and when it is feeble, many months more, or years may elapse, ere the woman becomes again pregnant. On the whole, therefore, it appears that there is no certain relation between periods of lactation and gravidity, and that the latter condition is dependent on constitutional peculiarities. But even did certain data establish the supposed protective influence of prolonged suckling, the end desired would not justify the means adopted, if it is found to endanger seriously the constitution of mother and child. On the other hand, supposing pregnancy should occur while a child is at the breast, as often happens, unknown for a considerable time to the individual herself, the effects on both are still more hurtful; or if, as is also very commonly the case, the mother goes on nursing for twelve, eighteen, or twenty months, or even longer, in the vain hope of averting a future evil, she is not only periling her infant's life, but as she may become pregnant immediately after the weaning, between alternate long-continued suckling and gravidity, her vital energies are so exhausted as to entail a train of the most disastrous consequences on her own constitution and on that of her offspring.

When a woman becomes pregnant during lactation, if other symptoms do not enable us to give an opinion, the microscope is a most important diagnostic guide; for then the colostric characters of the milk are apparent, and thus we can, without hesitation, advise an immediate abandonment of suckling.

The consequences of unduly prolonged lactation are sufficiently well known to render a lengthened statement unnecessary. The generality of women are in the possession of the best of health, and in the enjoyment of the greatest amount of earthly happiness while nursing, unless they are oppressed with much fatigue, or harassed with anxiety or other ills of life. It is a natural function, in the performance of which the warmest sympathies and liveliest emotions are excited, and a tone thus imparted to the physical constitution as well as to the mind. Nor should the good effect of lactation, properly performed, cease with weaning. It is permanent, and tends to the prolongation of female existence; but when continued beyond the term which nature has pointed out, it exhausts her strength, and, according to the character of her constitution, so are the evils resulting more complicated, severe, or lasting. From a consideration of this, it is easy to perceive that an offspring reared from an exhausted body, must, like the plants of a worn-out soil, pine away a sickly and delicate existence. The most common evils entailed on a nurse by this practice are severe headaches, giddinesses, blindnesses, inordinate and irregular action of the heart, loss of appetite, general debility, lowness of spirits and hysterical affections, sinking feelings, pain in back and loins with dragging sensations, leucorrhœal discharges, and menorrhagia. Such symptoms variously combined, and many others, may exist during suckling,

and may continue long after weaning, inducing severe functional disorders, or incurable lesions of the most important organs, more especially of the brain and uterus. Then as regards the child, during the first nine months of its existence, no kind of food answers so well for the purposes of growth, nutrition, and the support of life, as that provided by the mother's breasts; but so soon as the fountain becomes impure, from being overdrawn, the most deleterious effects are produced, where formerly there was apparently the best of health. If children thus exposed are not sustained by a great deal of nourishment otherwise, the consequences are soon displayed in their meagre bodies, pale countenances, in the vomitings, purgings, cryings, spasms, or convulsions; and subsequently these victims drag out a miserable life, or fall before the first accidental illness or epidemic malady with which they are assailed. I am quite convinced that a rigid inquiry into the proximate, as well as ultimate causes of infantile and maternal death, would afford a melancholy return in regard to the consequences resulting from ignorance and culpable imprudence in the matter of lactation.

The milk of those who overnurse presents the diseased characters already described, varying in different cases, not in kind, so far as I can discover, but only in degree. These are well depicted in Fig. X. 1 and 2; and it will be seen that, in addition to the colostric granules, the most important morbid change is in the reappearance of large oleaginous globules, and in the excessive agglutination of all the bodies together. When specimens are first placed on the glass under the microscope, there are scarcely ever any independent motions seen among the globules. Example No. 1, was taken from a patient who had nursed one year, and who, during the last three months, had become pale, thin, feeble, dyspeptic, and affected with psoriasis. Her child, on the other hand, was evidently ill-nourished, was latterly much troubled with diarrhoea, and had had several convulsions. Example No. 2, is the milk of a patient who was in the twenty-second month of lactation. She had been weakly for upwards of two years in consequence of repeated attacks of ague when in Canada West, and was now much emaciated, troubled with cephalalgia, palpitations, pains in the back, downbearing sensations, and was menstruating every three weeks. Her child, although obtaining a good deal of supplementary nourishment, had had much bowel complaint, was pale and flabby, and had not been as yet able to walk. Fig. VII. 1 and 2, although designed to illustrate the bad quality of the secretion obtained after weaning, in contradistinction to the character of the fluid found in the breasts in the early months of pregnancy (*Vide* p. 72), it may be worth while to notice here, that the individuals from whom the specimens were taken, may be adduced as proofs of the evils resulting from prolonged lactation. The former had been confined one year and a half previously, and nursed for fifteen months. She menstruated

once prior to weaning, and had done so every two weeks since. She suffered chiefly from pain in the back and debility, and is yet in very infirm health. The latter had been confined two years and a half previously, nursed for fourteen months, and ever since had been losing flesh and strength, had an abortion six months ago, since then almost constant menorrhagia, and is now undoubtedly affected with phthisis. The children of both parents, I may also notice, have been extremely weakly. Many such examples might be added to these; but I think enough has been brought forward in evidence of the bad consequences of prolonged suckling. The physician cannot too decidedly set his face against a practice so disastrous, or be too careful in his warnings to young and inexperienced mothers.

It has been supposed that the milk, which in its normal state is distinctly alkaline, may become acid in the breast, and consequently injurious to infant health. No data, however, have been given for this opinion; and I am inclined to think that acidity can only occur when the milk is removed from the breasts, and allowed to stand for some time before it is examined. I would thus also explain the appearance of the beautiful confervoid vegetation, the *penicillium glaucum*, which springs from the milk globule. M. Turpin has imaginatively conjectured this growth to be the cause of the knotted condition of an engorged breast, germination having taken place in the lacteal ducts from undischarged milk. The idea cannot, however, for a moment be entertained. The milk newly drawn is invariably alkaline, it soon becomes neutral, it ere long is found distinctly acid, ferments, is decomposed, and then readily gives origin to confervæ and vegetable infusoriæ.

By the microscope we are enabled with certainty to distinguish poverty from richness of milk;—the former by the sparing number and small size of the globules, compared with the quantity of the serum in which they swim; and the latter, by the large size and number of the globules. The naked eye cannot inform us satisfactorily whether, in a particular instance, the secretion is too thin to afford sufficient nourishment to the infant, or too rich for easy and healthful digestion. Nor can the healthful or unhealthful aspect of the nurse in every instance warrant more than a guess as to the quality of her milk, or her ability to furnish adequate supplies. Microscopic examination, however, will give decision to our impressions, and qualify or corroborate the opinion which we may have been inclined to form of the capacity of a mother to suckle, wholly or partly, or of the qualifications of a nurse as a substitute, and thus solve occasionally very great difficulties, and protect most important interests. The general considerations which ought to guide us in the choice of a nurse are of course not to be overlooked. If the mother has a good constitution, but is from any cause disqualified from suckling her own child, then we should look for a nurse as nearly re-

sembling her in constitution and physical appearance¹ as possible; but if, on the other hand, she is delicate or impaired in health, and if there is any occasion to suspect that she or the child's father have a hereditarily defective or tainted constitution, then it is imperative to choose a nurse of an entirely opposite mould. Hence it is that a change of milk is often called for, and a sickly child, or one in an almost hopeless condition, may be quickly restored to health by a timely transference. A nurse, too, should be chosen as nearly of age to the mother as possible, avoiding the extremes of maternal prematurity, or the period when a woman generally ceases to bear children. That the age of the milk also should be especially attended to, may be gathered from the preceding pages, as it has been shown that the qualities of milk vary materially at different periods, in its adaptation to or unfitness for the wants of the child. It is always improper to give a newly-born infant to a nurse when her milk is several months old. When such is the case, the child seldom thrives, and is apt to become afflicted with rachitis, or some other ailment of imperfect nutrition. Dr Combe, in his excellent treatise on the management of infancy,² states the general qualities which are indispensable to constitute the good nurse, as "sound health, a robust constitution, freedom from any hereditary taint, cheerfulness of mind, orderly, neat, and temperate habits, patient kindness and good-humour, and above all, spontaneous activity, and a strong and innate liking for children." He also mentions that the external appearances should be "moderate plumpness, fresh and clear complexion," clear cheerful eyes, with well-conditioned eyelids, deep red-coloured lips without cracks or scurf, sound white teeth, and well-formed moderately firm breasts, with nipples free from soreness or eruption."

External appearances are, however, often deceptive; and as the main requisite in a good nurse is the supply of the life-supporting element, a microscopical examination of it is recommended as the surest and easiest method of testing its suitability, especially when there is any ground for doubt. Our treatment of particular cases also, is thus more rationally guided. If a particular specimen of milk is found to be poor in quality, a consideration of the chemical and structural constitution of the secretion in its normal state suggests, that aliment which will supply the fatty and albuminous principles in abundance, will be best fitted to secure in the nurse the elaboration of a good secretion. This will be best accomplished by a mild vegetable and farinaceous diet, and of soups in which are suspended a considerable amount of oleaginous matter. As a proof of the capa-

¹ Children of tall spare women are said never to thrive on the milk of those who are short and stout.

² Edition 1847, pp. 74 and 76.

³ Too much weight, I believe, has been attached to the complexion of nurses, as to whether dark or fair women are preferable. Those who present a medium aspect are the best, always keeping in view the other qualifications desirable.

bility of the former articles to afford good milk, I may instance the dainty food which the cow and the sheep can furnish to their young ; and of the fitness when both are combined, I may instance the success with which the countrywoman can perform the function of suckling. All experience demonstrates that strong meats and drinks taken during lactation, beget a heat and irritability of the system, if not feverishness, and thus lessen rather than increase the amount and suitableness of the secretion. In these remarks, I do not mean to aver that solid animal food and stimuli should never be taken by nursing women. Habit, and many other considerations, must render a considerable allowance necessary ; but I mean to caution against the immoderate use of a highly nutritive and exciting diet, and a dependence on such from mistaken ideas of expediency. The benefit derived from stimuli in general, I very much question. They induce an unnatural excitation of the nervous and circulating systems, communicate only a forced strength to the constitution, which ere long leads to increased supplies to maintain it, and ultimately to great physical, mental, and moral evils.

When the milk is found too rich for the digestion of the infant, and is creating disorder of its stomach and bowels, it is of importance to give diluents freely, such as barley water, or thin gruel prepared from groats ; the latter of which is particularly beneficial, especially in the earlier days of suckling. It is of great consequence also, to attend to the state of the breasts in all instances of faulty secretion. In cases of too great richness of the milk, it has been observed by M. Péligré,¹ that allowing it to remain longer than usual in the breasts, tends to an increase of its serous or watery part, and therefore the child should not be applied in such cases very frequently. Regarding the general management of the breasts, I have already, in the foregoing pages, thrown out a good many hints, more especially as to the prevention and treatment of engorgements. In the case of weakly and unproductive nurses, a great deal may be done to improve the quantity and quality of the milk, by due attention to the regularity and frequency with which the breasts are given, and the amount of supplementary supplies which may be necessary for the infant at different stages during lactation. Although many interesting topics connected with the above subjects, and the influence of exercise, mode of life, &c., on successful lactation, yet remain for discussion, these must be set aside for the present, as I have already exceeded the limits permitted me.

EXPLANATION OF THE PLATE.

Figures magnified 206 diameters.

- I.—Example of healthy milk at the best period of nursing—mother and child being in good health.
- II.—Fluid taken in the third month of pregnancy.

¹ Quoted by M. Donné.

- III.—Fluid taken in the fourth month of pregnancy.
- IV.—Fluid taken in the seventh month of pregnancy.
- V.—Fluid taken in the ninth month of pregnancy.
- VI.—Milk three days after parturition, showing the characters of the colostrum, the appearance of the granular bodies in their perfect state, and the stages through which they pass in disappearing.
- VII.—No. 1. Fluid taken five months after weaning; patient not pregnant.
No. 2. Fluid taken one year after weaning; patient not pregnant.
- VIII.—No. 1. Milk of a patient in the fifth day of fever; the secretion formerly healthy.
No. 2. Milk of a patient in the eighth month of suckling, labouring under severe dyspepsia and general debility; child affected with vomiting and diarrhoea.
- IX.—No. 1. Milk of a patient in the sixth month of lactation; her symptoms being emaciation, dyspepsia, an eruption of the *molluscum contagiosum*, and ability to suckle only with one breast, the other having been early destroyed by suppuration. The child was affected with the same eruption, and presented the general appearance of being ill nourished.
No. 2. Milk from a breast which had suppurated repeatedly, and finally closed up ten days previously. Mother ceased to suckle at same period, after having nursed fourteen months,—but not constantly with the diseased breast:—child weakly.
- X.—No. 1. Milk of a patient who was in the twelfth month of suckling. She was thin and debilitated, while the child was affected with diarrhoea and convulsions.
No. 2. Milk of a patient who had nursed for twenty-two months; herself extremely weak, affected with cephalalgia, loss of appetite, &c.; and the child pale, flabby, and unable to walk.
- XI.—No. 1. Examples of fluid taken from the breast of a *female* child three days old.
No. 2. Example of fluid taken from the breast of a *male* child six days old.
- XII.—No. 1. Example of *Kiesterin*, with crystals of the triple phosphate of magnesia.
No. 2. Appearance of the greasy scum from the urine of an infant nourished solely from the breast.

ARTICLE II.—*On the Employment of Gutta-Percha in the Treatment of Club-Foot and Fracture.* By WILLIAM LYON, Senior Surgeon to the Glasgow Royal Infirmary.

EVERY one who has attempted, is aware of the difficulties which attend treatment of the varieties of club-foot.

The apparatus of Stromeyer, splints of wood or leather, retained by starched or common bandage, embedding in plaster of Paris, or use of shoes with iron soles attached to leg, thigh, and pelvic bands, are some of them expensive, others painful, and apt to occasion ulcerations; while they are all exceedingly uncomfortable to the patient, and troublesome for the surgeon to apply, adjust, and remove, operations requisite every two or three days during a considerable part of the treatment.

So much time and attention are required with any of these, that unless liberal remuneration can be given the surgeon must make

great sacrifices. The steel apparatus, with exception of expense, probably combines the greatest number of advantages, but still much time and trouble are required for its management; while, if it cannot be afforded, the unfortunate sufferer has too often to be left to his unhappy fate.

The method by imbedding the member held in the desired position in a quantity of Paris plaster, with one exception, answers remarkably well. The mould quickly becomes hard, the pressure by the resisting parts is equally diffused, and ulceration, and all but the most trivial pain, are avoided.

We have then, by this plan, facility of application, ease, efficiency, and cheapness; but there are the objections,—that the weight of the mould necessarily occasions confinement of the patient to bed, and that it is difficult of removal; a process frequently required for regularly and gradually restoring the foot and limb to normal relations. As substitutes I have thought of a bandage of lead, zinc, or the like, applied over the foot and leg, well protected by a stocking or otherwise, but have not put them to trial.

Keeping in view, however, the mechanical requisites for treatment of this deformity (club-foot), viz. diffusion and equality of pressure, facility of application, removal, and efficiency; it occurred to me that the recently introduced article, gutta percha, possessing pliability and adhesiveness when warm, hardness and strength when cold, would combine all these particulars.

I have now employed this article very successfully in several cases, seen it, on my recommendation, used in others, and write this to bring it under the notice of the profession; convinced, after numerous trials of all the other methods, that it is by far the most manageable and effectual, and that it will enable every one possessed of even the least dexterity successfully to conduct the treatment of club-foot, thus getting rid of the cumbrous, complicated, troublesome, and expensive apparatus in ordinary use, and enabling us to confer equal advantages on the poor as were mainly confined to the rich, and surgeons in situations remote from instrument-makers, with the utmost facility to treat these species of deformities.

It may evidently be applied in various ways; but the method I have followed is this,—they were all cases of *Talipes varus* in both feet, and after dividing the tendo-Achillis, a procedure probably not required in very slight cases, but if not indispensable, at least greatly abbreviating the treatment in severe ones, a bit of plaster was put on the wound, and a roller from toes to knee for protection of the soft parts.

A bandage of gutta percha, thickness of a penny piece, about an inch more or less in breadth, proportioned to size of limb, softened in nearly boiling water, and dried by gentle pressure between folds of a towel, was then quickly made to enwrap the limb from toes to knee, in usual manner of the common roller.

The limb is thus encased in gutta percha, and while the material is still soft and adhesive it is firmly and equally kneaded by the fingers, so as to mould it closely to the parts, and cause the turns of the roller to adhere to each other. The limb is now firmly held below the knee, while the foot is gently twisted outwards, with the toes in the same direction, and upwards. The desired position is maintained until the material becomes cold, which it does in a few minutes, which time may be shortened by immersion in cold water; when the *light, hard, equal, strong*, mould thus formed, effectually prevents the return of the foot to its abnormal position.

In a few days the gutta percha is removed, a matter easily accomplished by regular unrolling of the turns of the bandage, reapplication is effected, a little further restoration of deformed parts being obtained; and by thus proceeding at intervals of a few days, the foot and limb are, in the course of two or three weeks, restored to their normal relations.

I suspect it is a considerable recommendation of this method by the material in form of a roller, that it is with the greatest facility removed, which I do not think would be effected without difficulty if the mould were otherwise formed. It would add greatly to the value of the practice could the material be easily softened, the parts adjusted, and removal avoided. This I have attempted, but the high temperature required to make the article soft and adhesive, rendered the effort abortive.

I have not experienced any bad effects from the advised practice; but I can readily conceive, if the mould be dimpled when soft, or otherwise applied unequally, pain, ulceration, or abscess must follow such mismanagement, although protection by a thick soft bandage, layers of carded cotton, or a stocking, will go far to prevent or remove them. So likewise, unless the urine be prevented by a piece of oiled silk from constantly soaking the bandage and skin, excoriation will ensue; and it has been said that confinement and accumulation of perspired matters will have a similar tendency; to which I can only reply that it has not happened in my cases, even when the mould has remained on for weeks, which it will very rarely require to do in club-foot.

The cause of the deformity, originally in the nerves of the limb, soon involves the muscular, ligamentous, and osseous structures, and is of course more easily overcome the younger the patient. I have repeatedly, without the least untoward occurrence, operated within a week after birth. The patients treated by the plan recommended were all infants (one excepted, who was three years of age), and it may be questioned whether the method is applicable to more advanced periods of life. From my experience by the analogous method with Paris plaster, I have not a doubt the gutta percha will answer for more advanced patients in the same degree as the more common modes of treatment; that is, that it will be found as efficacious and manageable, if not more so, than the others; but that it

will be necessary to proportion strength of the mould to the resistance to be overcome, a matter easy of accomplishment by several coatings of the gutta percha roller.

There is one drawback to the plan, but it is more in appearance than reality. The articulations in the foot and at the ankle are rendered immoveable, and assistance in throwing upwards and outwards the foot by the weight of the body in standing and walking is lost. This objection does not apply in the cases of very young infants who are unable to stand; and the paralyzing effect of perfect repose on the overacting muscles, and the gain by the contraction which is permitted to the opposite and debilitated ones, has rapid effect in restoring balance of action. At all events, it is easy, when the difficulties of removing the deformity have been overcome (by far the most troublesome and painful period of the treatment), to prevent relapse by exchange of the gutta percha for such an apparatus as will allow the muscles to be called into action, the articulations to play, and the weight of the body in standing and walking to aid in the perfect restoration of position and muscular action.

It must not be overlooked that in club-foot (Varus at least) the disease and consequent deformity are not confined to the foot. The whole limb is generally affected, the leg and thigh are rolled inwards; and though the sole may be turned down, if means be not taken to counteract the involution of the other parts, the patient will nevertheless walk with the toes of each foot directed completely inwards; the one requiring to pass over the other at each step, as is often seen, or if existing in a lesser degree, will constitute the ungraceful state, called *in toes*. This is obviated in the treatment with the usual steel apparatus, by fixing the leg straps at a proper point on a metal band which passes round the pelvis. A similar result is obtained in the method I recommend, by placing the feet in a pair of boots, with the toes directed outwards, as in the "first position;" the boots being retained in the proper direction by being sewed on a piece of sole leather, and worn constantly or occasionally, as may be thought requisite. In this way one set of muscles is elongated and debilitated, another abbreviated and strengthened, as by the gutta percha on the foot and leg, and balance of power is in a short time obtained.

By perseverance in such manner for several months, I have never failed in young patients to restore healthy form and action of parts. But if the surgeon is not backed by determination on the part of the parents, and the treatment continued until perfect balance of muscular action be obtained, all our efforts will be lost, and a deformity which most seriously affects the comfort, interest, and status of the patient be unalterably entailed.

In reflecting on the treatment of this disease, and the difficulties met with by the ordinary methods; and seeing that the spasm or overaction of one set of muscles is the original cause of the displace-

ment which ensues, and the principal obstruction to restoration of form and action; it occurred to me, that if the morbidly acting muscles could be temporarily paralysed, their effects, an altered conformation of bones and ligaments, would with comparative facility be overcome. Pursuing this reflection further, and recollecting what is observed occasionally in sciatica, viz. paralysis of the muscles of the entire inferior extremity; and coupling this with the fact of reunion of divided nerve and restoration of function, and likewise that the sciatic nerve may be divided without hazard of injury to important parts: I have often put the question to myself hypothetically, would not division of the sciatic nerve by sub-cutaneous section be an easy, safe, and effectual means of overcoming the difficulties attendant on treatment of club-foot?

The anatomy, physiology, and pathology of this nerve would lead to the conclusion of its beneficial division in club-foot; but it must be remembered, it is composed of sensory as well as motory and other filaments; and observing the occurrence of sloughing from weight of the parts in paraplegia in injury to spinal marrow, a fear arises whether the like might not occur from division of the nerve; or at least that the parts would not with impunity suffer the requisite pressure for overcoming the osseous and ligamentous obstructions to restoration of form. Besides this, for any thing known to contrary, the abnormal functions of nerves and muscles might be reproduced after reunion.

Trials on the inferior animals are necessary to decide as to the consequences of what, I am aware, many will esteem an uncalled-for, unfeasible, and absurd proposal.

The obtention of a *light, strong, equal, hard* mould, quickly and easily formed, as in the method above described, suggested to me the gutta percha as a substitute for the leather splint and starched bandage, which I had often employed in the treatment of fractures.

The slight excitement which usually follows immediately after reception of a fracture having subsided, it must be the opposite of advantageous to derange and debilitate the system by five or six weeks' confinement to bed; and any plan by which the patient can be invigorated by exercise, while the injury and irksomeness of long and perfect rest in bed are avoided, is surely an important gain.

All these purposes are most effectually served by the gutta percha.

In several cases of simple fracture of the bones of the leg, the *excitement* and *swelling* having subsided under attention to perfect repose, elevation of the member, and use of the ordinary roller, lateral cushions, splints, &c., I have most advantageously practised the following method.

The foot being surrounded by a common roller, and the leg with a scultetus, adjustment of the fracture is maintained by extension; while at same time (whether beginning above or below is unimportant) the limb is encircled by the turns of a properly softened gutta

percha roller (that is, of a strip of the material two or three inches broad,) applied edge to edge so as to avoid inequality in the mould, which might irritate when it becomes cold and hard. Over this, a similarly prepared roller is passed from toe to knee, which, adhering to the one below, and to each of its own turns, forms in a few minutes (a great advantage over the starched bandage and leather splint) an equable, light, strong, hard case for the limb, completely preventing motion at the seat of fracture; and in fact acting, like the shell in some of the lower animals, as an *external* bone, within which the soft parts, and in this manner in fracture the broken bone, is protected, and effectually prevented from displacement.

If the patient were to be confined to bed, this method would be applicable to fracture at any part of the leg; but its principal recommendation is, that the patient by it is not necessitated to keep his bed. It will be evident, however, that the plan is not likely to be equally efficacious in all cases, as for instance in those near or in the upper third, where the upper fragments being short, and the lower long, consequently affording by weight a powerful lever, displacement will be hazarded; but may be prevented by extension of the mould to a portion of the thigh.

As to displacement by shortening, this may be prevented by close application of the mould below the broad head of the tibia, which will prevent it slipping upwards; and although it may be supposed that the mould will revolve, and thus alter the proper relation of the fragments, this does not happen, unequal figure of the limb preventing it.

In a week after reception of the fracture, the patient may thus, by a few *minutes'* exertion of the surgeon, be enabled by his own efforts to throw his limbs out of, and sit on the side of the bed: or the limb may be raised and allowed to fall on the bed, without injury, and in fact without pain, to the patient. He may now be allowed to turn from side to side, to walk with crutches, the limb being slung from the shoulder; conditions, the comfort of which, opposed to the ordinary long, irksome, and to the business man very inconvenient, and to all debilitating, confinement to bed, need not be dwelt upon: the advantages of which in shortening and perfecting osseous union, will be generally conceded: and peculiarly valuable, where transport of the patient for considerable distances is requisite: a circumstance recommending this method in military practice, where unavoidable conveyance, with inadequate means, in countries without roads, or very bad ones, is frequently demanded.

In fractures of the bones of leg and forearm, and in that of arm, the eligibility of the gutta percha is very evident; whether applicable to the femur is more questionable: though if the limb were contained in a tightly fitting, *light, strong, hard case*, oblique or transverse displacement would be prevented; and if the case extended to the calf or knee, and to the nates, perineum, ischium, and external aspect of pelvis, the inequalities of the limb below, within the case, and the

resistance to the latter by the pelvis above, would, from the pyramidal form of the contained parts, render displacement by shortening unlikely: trial, however, must determine the point, and I shall take the first opportunity of testing it.

Since writing above, I have employed the gutta percha in the case of a boy with fracture at junction of superior and middle thirds of femur. Extension being made, the material was applied over a roller from toes to upper part of thigh, close up to the perineum, round the pelvis, and again upon thigh and pelvis until the nates of injured side were completely covered. Immediately after, he could without uneasiness be turned from side to side, and carried in his mother's arms, and I doubt not could have readily walked with aid of crutches.

Indeed the thigh and pelvis were immoveably connected, they and the leg might be said to have been in a few minutes converted into a hollow bone, in which the nates, perineum, and tapering form of the limb above, with confinement of the foot in the mould below, entirely removed fear of longitudinal, while the tightness of the case prevented lateral, displacement. Further trial is necessary before inferring a general rule; but certainly the application had a most promising appearance, and enabled the patient to enjoy a degree of ease, and amount of motion, quite new to us in fracture of the femur of a few days' duration.

Several theoretical objections may be made to the practice, such as, that the material being applied warm, it will have contracted when cold, so as to cause injurious compression; that its frequent removal and reapplication will be troublesome and prejudicial, and yet without them the state of parts cannot be observed. None of those occurrences common to this and the treatment by starched bandage, have presented to me, and, if the recommendations are attended to, I do not think they need be feared; while some of them can be prevented, and the others are greatly counterbalanced by many advantages.

I have the method in practice and prospect in several other conditions. Six years ago, a girl was under my care on account of acute necrosis of tibia. Almost all but the epiphyses became loose, was removed by incision, and the limb then kept straight and steady by splints.

At the end of several months osseous matter was deposited throughout all the space from which necrosed bone had been removed, excepting about an inch in length in the centre; where the small apices of the pyramidally shaped portions were connected with soft tissue as in ununited fracture, and, in consequence, the limb has remained useless ever since.

Amputation was proposed by one party, removal of the soft tissue, and repose as in ununited fracture, by another. In mean time, the method of breaking up intervening soft tissue, in non-union of fracture by Professor Miller, being observed; I determined to give it a trial in the analagous circumstances of this case. The breaking up

was very freely executed, and the limb then put up in gutta percha as described, where it has remained free of uneasiness for eight or ten days, and, at least, in such repose as to afford the most favourable local condition for the practice, while vigour is retained by the free exercise permitted.

This girl has had a wooden sole and pin attached to the gutta percha case, the diseased limb being shorter than sound one, and she now walks freely about the ward with assistance of a stick. She is enabled to do this by the weight of body being thrown on the broad part of the tibia within the case, while the hardness of the latter prevents the foot being pushed upwards by pressure from the sole; thus showing another important application of gutta percha, viz. for the formation of a box to receive the stump after amputation in the leg, thigh, fore-arm, or arm. This can be made in a few minutes at a trifling expense; will be light, easy from equality and diffusion of pressure: sufficiently strong: it can be made of any degree of strength: and can be prolonged to the proper extent, either by a pyramidal coil of the same material, or by attaching a wooden cup and pin.

For long stumps of the thigh or leg, I have a strong conviction of the great efficacy of this method; our able instrument-maker, Mr W. B. Hilliard, joins in my opinion: the case of the girl demonstrates it: and I have a strong hope, that to the poor and unfortunate labouring class, who principally require such assistance, it will be found an important acquisition.

A few days ago, I dissected off a flap and removed a detached fragment, three or four inches long, which lay in the cancellous structure at the point of contact of the two fragments, in a case of compound fracture of the tibia, which has been under treatment for six months, and is still ununited. I likewise pared the smooth rounded extremities of the fragments, and, when the excitement has passed off, intend to put up the limb in gutta-percha. By the perfect repose in which, the improvement of the debilitated frame by exchange of exercise for the long confinement to bed, and the removal of the dead fragment which acted like a foreign body, I hope yet to avoid recourse to amputation.

I have likewise the material under trial for preventing motion in diseased articulations, a mode of treatment now universally commending itself in theory as in practice; and here, too, the article bids fair for supplanting the methods of accomplishing the purpose hitherto in use. When employed with this view, its application in form of roller is much superior to that of splint, the latter permitting some degree of motion.

Many other applications of this pliable material suggest themselves; but these trials and propositions will suffice at present for attracting attention of practitioners to its various and important uses in surgery.

July 16.—To-day I applied the gutta percha in another case of fractured femur. The woman, thirty-three years old, was received into hospital a week

since. The material was applied as in the case of the boy detailed above; and, in ten minutes after application, she was raised to the erect posture, walked from the side-room without pain into the ward with aid of crutches, and would have walked to her bed but for faintness. I did not allow her to bear on the injured limb; but from the great strength of the mould formed as described, and the pyramidal form of the hip and thigh, the large circumference being above, fitting into, and bearing upon the hard case, I am sanguine enough to hope that the time has passed for the necessity of confinement to bed in fracture of the femur; and that not only will the patient, in favourable cases, be allowed to turn from side to side in bed, and walk about on crutches, but that he will, with impunity, throw a considerable part of his weight upon the mould, as one who has suffered amputation in thigh, does in the ordinary box of the artificial limb.

The first opportunity which offers, I shall try a different method of putting on the material, the one described being troublesome, and requiring several assistants. I shall shape two pieces of paper sufficient to cover the member and pelvis from each side to an extent equal to that enveloped by the gutta percha in the former cases. These being softened in warm water, will be laid on each side of limb, and behind and on outside of pelvis, their edges slightly overlapping, and will be then surrounded with a common roller; an accurately fitting, close, hard, strong, light mould, will thus be formed, in which, from the resistance above and below, the limb cannot shorten, which, tightly compressing the soft parts, will also prevent lateral displacement or riding of the fragments, and I am hopeful, will enable the patient partially at least to use it in walking, the weight of the body being thrown on the upper part of thigh, hip, and perineum. What the effect temporarily of this treatment as respects speedy and perfect union, &c. &c., remains to be seen; but the following note, just received from the gentleman acting as my clerk in hospital, may prove interesting:—

“DEAR SIR,—I again saw the woman walking with the aid of crutches; she walked half the length of the ward with comparative ease, and almost no pain;—with no assistance but the nurse holding her gown. She, however, requires some lessons in crutch-walking; otherwise the result is highly satisfactory. Complains of a little pain from tightness in groin. She soon became weak, and had to return to bed.—I remain yours truly, GEORGE BUCHANAN.”

ARTICLE III.—*Contributions to the Pathology of the Kidney.* By WILLIAM T. GAIRDNER, M.D.

PART I.—PATHOLOGICAL ANATOMY. (Continued from p. 21.)

III.—LESIONS OF THE TUBES AND EPITHELIUM.

SOME of these lesions have been already fully described under the head of exudation (I.); but there remain others which are not less important in themselves than those formerly alluded to, and which are very frequently found in connexion with them.

Imperfect Development of the Epithelium Cells and Nuclei.—The natural condition of the epithelium cells has been fully described in the anatomical introduction to this memoir.—(April No. p. 720). The size of these cells, and the thickness of the cell wall, has been stated to vary within certain limits in organs apparently healthy. The size of the nuclei is less variable than that of the cells; but in all kidneys, whether healthy or diseased, the nuclei which are most closely adherent to the basement membrane are less perfectly circular, and of considerably smaller size, than the majority of those lining the tubes, and surrounded by complete cells.

Notwithstanding these differences in the normal condition, the physical characters of the epithelium is capable of affording important information as to the diseases of the kidney. In very many pathological conditions of the organ, the nuclei occur in various places almost wholly devoid of cell walls. They may be more abundant or more scanty than usual; and often appear in great profusion, huddled together in confused masses, and mixed with shreds of membrane and amorphous molecular matter, not soluble in acetic acid. This appearance of debris, which no doubt results from disintegration of the cell walls, most frequently occurs in kidneys which are abnormally soft and large, and from the cut surface of which an unusually large amount of turbid whitish juice may be scraped. It is usually impossible to obtain a satisfactory microscopic section; the cohesion and elasticity of the organ being so much impaired as to present no resistance to the pressure of the glasses. Such softened and altered kidneys occur frequently in fever and other diseases, and have appeared to me frequently to concur with deficiency in amount, or alterations in character, of the urinary secretion. I have not been able to arrive at any very definite conclusion, as to how far the disintegration in question may be the result of post-mortem change. However this may be, it is clearly abnormal, and ought always to suggest careful examination, as it is undoubtedly often connected with other morbid characters.

A more unequivocal pathological change (often occurring along with the above) is the small size and altered form of the nuclei throughout the organ. I have frequently observed the majority of the nuclei to be not more than half the usual size (some of them being even less); in this case they have always been destitute of cell walls, and have presented a more or less oval or slightly angular form. Sometimes they float scattered and solitary in the field of the microscope; at other times they appear aggregated together, either by two and three, or in much greater numbers. When a few of these aggregated nuclei are observed, it can usually be seen that they are not actually in contact, but are inclosed in a very delicate and transparent filmy substance, which is readily twisted about in all directions by currents in the fluid, but which, nevertheless, has sufficient tenacity to prevent the nuclei from being torn asunder. Occasionally, in the midst of this connecting substance, obscure marks of cell walls can be observed around the nuclei; and, from repeated observation of these varieties, I am convinced that this transparent and homogeneous film is nothing else than the nascent or undeveloped cell membrane, which has separated from the basement membrane along with the half-developed or young nuclei above detailed. These aggregations of young nuclei are sometimes mingled with the amorphous debris of effete epithelium, or with granules and molecules of oleo-albuminous exudation, or of lithate of ammonia, which communicate to them a dark and confused appearance. Not unfre-

quently also these masses, when freed from the tubes, retain more or less of their form, and present so exactly the appearance of the casts of the tubuli seen by Franz Simon, and many other observers, in the urine, as to leave no doubt of their identity with these bodies.—(See Figs. 17, 18.)

Desquamation of the Epithelium.—The changes above described are generally accompanied by an extremely rapid generation of nuclei, which are separated from the basement membrane in an imperfect state, and carried away along with the urine. I shall not at present enter into the subject of the changes in the urine, further than to say, that the appearance in that fluid of the immature nuclei and cells, as well as the aggregations above mentioned, forms one of the most sure and undoubted signs of a diseased condition of the urinary tubules. There is reason also to think that disease may be detected in this way, long before the kidney has undergone disorganization at all evident either to the unaided eye or to the microscope.

The analogy of the anatomical changes now described, with those which occur in diseased mucous membranes in general, deserves to be adverted to in this place. Henle was the first to show satisfactorily that the essential phenomenon of mucous catarrhs and inflammations is usually the increased formation of epithelium cells in various stages of growth, and their separation along with an increased quantity of fluid secretion. The same fact has also been clearly elicited by Lebert, in his examination into the microscopic character of expectoration.—(*Physiologie Pathologique*, Vol. I.) The analogy of the phenomena in these cases, with those presented by urine in the various disorders of the kidney, in which desquamation takes place from the tubuli, cannot fail to be appreciated, when it is remembered that the basement membrane of the tubuli is essentially a mucous structure, differing only from mucous membranes in general by its anatomical arrangement, and by its containing none of the accidental or non-essential parts of mucous tissue.

The desquamation of the epithelium of the tubuli uriniferi, has been recognised by several continental pathologists as an important characteristic of renal disease. Vogel (*Icones Histologiæ Pathologiæ*, p. 108), in particular, has furnished important observations on this subject; and several other authors might be cited as having alluded to the different forms and sequelæ of this morbid process.

In the memoirs of Dr Johnson and Mr Simon, published simultaneously in the London *Medico-Chirurgical Transactions* for last year, the desquamation of the epithelium and its anatomical results, are described, from independent observations, as characteristic of the inflammatory affections of the kidney, and as distinguishing these from the chronic *fatty* degeneration of the organ. According to Mr Simon, the latter is the exclusive result of scrofulous disease; while the desquamative disease is the consequence, in general, of a rheumatic or other *inflammatory* diathesis. Dr Johnson describes

the desquamation as occurring both in an acute and chronic form, to which he gives the names of *acute* and *chronic desquamative nephritis*; and these are held to be true inflammatory affections, giving rise to organic changes of a peculiar kind. In the description of these changes, and of their relations to the different forms of renal degeneration described by former pathologists, there are considerable differences between the memoirs of Dr Johnson and Mr Simon, some of which will be presently adverted to.

In detailing the results of my observations on the anatomical changes in the kidney, I have avoided as much as possible all speculations as to the pathological causes or symptomatic phenomena of the lesions described. This branch of the subject I hope to take up at a future period; in the mean time, however, it is necessary, to prevent misconception, that I should repeat as regards the alleged scrofulous and rheumatic forms of renal degeneration, what I have formerly said in reference to the inflammatory and non-inflammatory,—that the application of such names to the anatomical changes in diseased kidneys is subject to this objection, that none of the lesions which I am engaged in describing, can be correctly considered as having an exclusive connexion with any specific pathological cause. In particular, I believe that there is no disorder of the system in general, or of the kidney in particular, with which the desquamative process is not liable to be connected, and that its connexion with the oleo-albuminous exudation is the most frequent of all.

I have therefore thought it necessary to exclude all names having reference to such vague pathological theories; believing that the lesions of an organ must be made the subject of purely anatomical consideration before its pathology can be rightly apprehended.

In some cases of desquamation of the epithelium, it is scarcely possible to recognise any departure from the usual condition of the kidney, either with or without the assistance of the microscope. The degree of vascularity is very various in different specimens, and the epithelium thrown off is so quickly resupplied, that there is no very observable change in the microscopic condition of the tubules. In one very intense case, in which ten pounds of very watery urine, loaded with an epithelial sediment, were passed daily for some weeks before death, the kidneys were small, flaccid, and bloodless; many of the tubes were quite full of nuclei heaped closely together; some of the nuclei were undersized; the cells, when entire, were much compressed and angular.—(See Fig. 15.) In another instance, where urine was passed in large quantity and full of epithelial debris, during the last two months of life, the kidneys were found in an opposite condition; viz. large and congested, and with a firmness and smoothness of section like the first stage of the waxy degeneration formerly described.—(*July* No. p. 14.) In this case the condition of the tubuli was in most parts quite natural; in some, however, there was extravasated blood, and in others the epithelium had accumulated to an abnormal extent. In both these cases there was

imperfect development of the epithelium; but cases have occurred to me, in which this character was by no means well marked. The crowding of the tubes with nuclei, although frequently found in the earlier stages of desquamation, is not invariably present; and I have seen the tubes gorged with epithelium, in a case where none had been separated with the urine for weeks before death.

Fig. 14.

Fig. 15.



Fig. 14.—A portion of a tubulus uriniferus normally filled with cells. It presents an obviously cylindrical form, and the nuclei are disposed on the internal surface so as to leave considerable intervening spaces. The cells and nuclei from such a tube are unaltered in form. (250 diameters.)

Fig. 15.—A tube crowded with nuclei and compressed cells. Some of the cells altered in form, and nuclei are seen mixed with debris, outside the tube. The nuclei are somewhat smaller than those of the healthy tube. (250 diameters.) Figs 14 and 15 are from the kidney mentioned in the text, (p. 99.)

So long, therefore, as the epithelium is freely regenerated, the kidneys may preserve a tolerably healthy appearance even on minute examination. The principal characters of the disease in this stage are derived from the urine. After prolonged disease, however, further changes take place. The epithelium becomes more sparingly generated, and is thrown off in the coherent masses above described (p. 97), leaving the basement membrane in portions bare, or with a few scattered oval nuclei, much smaller than those cast off, adhering to its inner surface.—(See Fig. 16.) In the microscopic examination of organs in this condition, there are frequently seen films of such exceeding delicacy and transparency as to be only visible by very careful management of the light. They preserve the shape of the tubules, and contain no nuclei or structures of any kind. Similar films are occasionally seen in the sediment of urine. They are probably thrown off from the denuded basement membrane.—(See Fig. 18.)

Obliteration of the Tubes.—The basement membrane, which, with the few closely adherent oval nuclei above described, is now the sole remaining structure of the tubes, soon undergoes a change.—(See Fig. 16.) It loses the cylindrical form proper to it in the fresh and natural kidney, and becomes flattened by the pressure of the surrounding parts. Its cavity is thus obliterated, and what was a tube assumes the appearance of a transparent riband, dotted here and there with small oval nuclei, which, when seen at the edges, appear

to be enclosed between two layers of membrane. These ribband-shaped portions of membrane appear to preserve considerable tenacity and elasticity; by their greater density, and by the constant presence of the small oval nuclei so often mentioned, between their layers, they are in most cases readily distinguished from the delicate films which have been referred to above. They are very various in diameter, but are always inferior in this respect to the normal tubes; and they appear to break up spontaneously into smaller portions, each of which contains from one to six, or even more, nuclei. These portions are of various sizes, from 1-10th to 1-4th of a millimetre in length, and from 1-120th to 1-30th in breadth. They are usually broadest in the middle, and taper to a point at both ends. The smallest of them contain only a single nucleus, and present an appearance in every respect like that of the young fibres of areolar texture, or those fusiform cells which have been called *fibro-plastic*. I think it probable that the whole of the diseased basement membrane ultimately splits up into fibres of this kind.¹

Fig. 16.

Fig. 17.

Fig. 18.



Fig. 16.—Fragments of tubes from which most of the cells have separated, and which are undergoing the change mentioned above. Some small nuclei are seen adherent, and others free. (250 diameters.)

Fig. 17.—Debris of epithelium separated from the interior of the tubes, and presenting a mould of their form. (250 diameters.)

Fig. 18.—Delicate films presenting the form of the tubes, and probably separated from their interior. Along with them a few nuclei and cells, which, however, are not imbedded in the membrane, as in Fig. 16. (250 diameters.)

While these changes are proceeding, the capillary vessels, which have ceased to be subservient to secretion, are usually obliterated. The consequence of this double obliteration of vessels and tubes, is a considerable degree of atrophy in the diseased parts; and, as the atrophy takes place at first chiefly in the cortical substance, great irregularities of the surface generally supervene. Thence arises the appearance so well described and figured by Dr Bright (Plate III.

¹ I have never seen any reason to believe, with Mr Simon, that the tubes in diseased kidneys burst from over-distension, discharging their contents into the inter-tubular tissue. The separation of the epithelium from the tubes, under the pressure of glasses, takes place to a considerable extent even in healthy organs, and much more in disease; but it is the result of the manipulation, not a pathological appearance.

Fig. 2), in which, from the atrophy of the cortical substance, the bases of the pyramids "are drawn towards the surface of the kidney."

When oleo-albuminous exudation supervenes on the above derangement of the tubes, or when desquamation supervenes on the former (circumstances which I conceive to be of very common occurrence), the exudation most commonly takes the form of the granulations of Bright, which are deposited chiefly in the diseased tubes; and the atrophy proceeding around these they become salient, and the surface generally irregular, giving rise to the tuberculated state of the surface, so common in all the later stages of the granulated kidney (Bright, Plate III. Fig. 1; Rayer, Plate VII. Fig. 6; Plate IX. Fig. 8.) As the atrophy, however, proceeds, the granulations are gradually absorbed; and when the kidney has become extremely contracted and irregular, they often in great part disappear.

The atrophied portions of the kidney are usually exsanguine and of a tawny or drab colour; they have considerable hardness and toughness. Examined microscopically, they appear to consist of fibres and fusiform cells in great abundance, and of more or less granular exudation, according to circumstances. According to Henle, Eichholtz, Gluge, and others, these fibres are in great part new formations; Johnson and Simon consider them as nothing more than the compressed parenchyma of the gland, from which all the other normal elements have disappeared. I look upon them as formed in great part by the breaking up of the basement membrane of the tubes (as above-described), as well as from the parenchyma and obliterated capillaries. It is not improbable, however, that, in addition to these elements, some new fibrous tissue is formed.

The extreme stage of the atrophied kidney is nearly the same whether exudation have existed or not.

Microscopic Cyst-formation.—It occasionally happens, on examining the section of a kidney with the microscope, that we see scattered through some parts of the section a few small clear vesicles of nearly circular or oval form; they are either of a very pale straw-colour, or nearly colourless, and are perfectly clear and translucent, with a very distinct shadowed margin, which causes them to stand out in bold relief from the other textures composing the section. Their diameter is usually from 1-40th to 1-15th of a millimetre, but in this respect they vary considerably; sometimes they appear to lie in the tubular areolæ, and at other times to be unconnected with these. Very rarely they have appeared to contain a few granules; most commonly, even when there is granular exudation around them on every side, they contain nothing but clear fluid. Their refractive power is not so great as that of oil, while it is much greater than that of the spherical cells of the tubes. Hence their distinct and characteristic shadowed outline.—(See Figs. 19 and 20.)

Fig. 19.

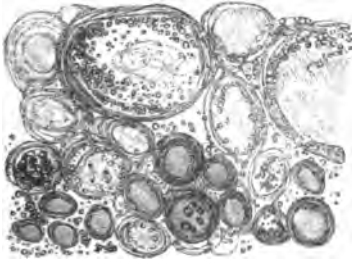


Fig. 20.



Figs. 19 and 20.—*Vesicles described in p. 102, dispersed amid the normal elements of the section of the kidney. There is a considerable quantity of granular exudation in the Malpighian capsules and tubular areolæ. One vesicle in Fig. 20 contains two or three granules, but all the rest are quite clear. In Fig. 19, a tube contorted, and here and there apparently much constricted, is seen to pass from the left (below) to the right (above). One of the vesicles lies over a constricted portion, and two others are seen in contact with the tube below.*

These bodies (which, however, have never appeared to me to present distinct nuclei) are probably the same with the “nucleated cells or vesicles” described by Mr Simon, as resulting from the extravasation of the epithelial cells into the intertubular tissue, and as progressively enlarging so as to form the cysts visible to the naked eye, which are so common in diseased kidneys. To these structures he attaches great importance in the pathology of the kidney, conceiving them to be the invariable result of the desquamative disease when of long standing; the kidney being, in Mr Simon’s opinion, changed more or less into an aggregation of microscopic cysts, which either undergo absorption, and lead to atrophy of the organ, or increase in size and monopolize its texture. Thus, according to Mr Simon, the serous cysts so common in the kidney result from an enormous development and hypertrophy of extravasated epithelium cells, which assume the character of the vesicles he describes, and acquire the power of increase and endogenous development.

Whether the bodies described by me above, are the same with the vesicles of Mr Simon, I have some difficulty in determining; but they are the only objects I have seen which correspond at all closely with his description, unless, indeed, it were possible to suppose, as Dr Johnson appears to hint,¹ that he may have mistaken the normal disposition of the tubuli (See Fig. 1, *April* No. p. 722) for a cystic structure.

However this may be, I am satisfied that the vesicles above described are exceptional productions, and by no means invariably connected, as Mr Simon describes his vesicles to be, with the progress of the desquamative degeneration. They are seen in comparatively few cases; on referring to four, of which I have drawings or memoranda, I find two to have been congested and waxy kidneys, with

¹ See first part of article *Ren*, in Todd’s *Cyclopædia of Anatomy and Physiology*, just published and not yet completed.

slight exudation, one to have been a soft and desquamating kidney, also with slight exudation, and one a granular kidney, with numerous cysts, from the size of a pea to that of a hazel-nut. On the other hand, I have examined organs in every stage of desquamative disease without finding these bodies, the production of which cannot therefore be an essential step in the degeneration and atrophy of kidneys so affected.

The origin and progress of these vesicles is very obscure. It is not improbable that, as Mr Simon asserts, they are transformed into the larger cysts visible to the naked eye; though I confess that I have not been able to trace the intermediate steps of their progress in a satisfactory manner. On the other hand, their origin from extravasated epithelial cells seems exceedingly improbable; indeed, I have already stated that I do not think the epithelium ever becomes extravasated. Moreover, the vesicles in question have all the appearance of being formed *within* the tubes, although they afterwards become separated from them.

From the occasional appearances of alternate distension and constriction presented by the tubes when undergoing obliteration, I am induced to believe that cysts may be formed by the occlusion and isolation of portions of tube which have not yet lost their power of secretion. Whether the vesicles in question are formed in this way, can only be determined by close and repeated observation; and I have not been able to obtain demonstrative evidence on this point. The observation in Fig. 19, however, though not free from sources of fallacy, appears to me to favour this view.

The larger cysts in the kidney present very strong evidence of being formed in connexion with the secreting membrane. In one instance I found their inner surface to be lined at some points with tessellated epithelium, in the form of pentagonal or hexagonal flattened cells, with circular nuclei; in another case there were oval nuclei without any distinct cells, and a large number of free oil-globules of considerable size. The existence of oil in these cysts has also been observed by Dr Johnson. Other products of secretion are also occasionally found. On one occasion I found several cysts in a kidney otherwise healthy in appearance, which contained a turbid ochrey-coloured liquid, presenting under the microscope numerous minute crystals of uric acid. Mr Simon mentions having found on two occasions xanthic oxide in considerable proportion. I have more than once observed them to contain blood in large quantity, and I have likewise found them full of a matter like stiff glue.

The occurrence of cysts in kidneys presenting a generally healthy structure is so frequent, as to lead to the idea that they must be in such cases the result of disease which has been arrested before any considerable disorganization has taken place. Many of the cases of partial atrophy of the kidneys figured by Rayer (see Plate V. Figs. 5, 6; Plate XXXV. Figs. 8, 9, 10), are probably due to the rupture or obliteration of these cysts.

Before leaving the subject of cyst-formation, I may state, that in one instance I have observed the Malpighian capsules to be occupied by distinct cysts. This case will be presently alluded to.

Dilatation and Thickening of the Tubes.—This condition, although by no means a very frequent one, is important as being characteristic, so far as I have observed, of the extreme stages of what I have called the “waxy degeneration.” I have scarcely ever seen it unaccompanied by entire obliteration of the vessels, and by enlargement and increased density of the kidney. The organ has the dense, resistant feeling of fibro-cartilage, and both cortical and tubular portions have the light yellow colour, and the appearances described in the July No., p. 15, as those of the waxy degeneration in its last stage. The striæ of the pyramids appear to radiate indefinitely towards the surface, and meet the cortical substance in digitations, instead of being marked off by a sharp semicircular line, as occurs in the healthy kidney. When examined with a simple lens, or even the naked eye, the pyramidal striæ are seen to pursue an unusually sinuous course; this is peculiarly the case where they pass into the cortical substance. Moreover the pyramids are unusually broad at the bases; and the length of the straggling digitations is sometimes so great, that I have measured fully an inch and a half between the extreme end of the striæ and the corresponding papilla. Nevertheless the cortical substance is not usually diminished in quantity, being developed to a great extent between the pyramids.

This condition I have ascertained to proceed from dilatation and thickening of the tubuli uriniferi throughout the organ. The dilated tubes are usually twisted and varicose, as may be seen by inspecting a section of the pyramids with a low power.—(See Fig. 21, A.) When examined with a higher power, the section presents an appearance very similar to some tumours (of the fibrous or fibro-cystic kinds); viz. a number of compressed areolæ, enclosed by fibrous tissue, and presenting an appearance of irregular concentric rings of various distinctness, (an effect apparently due to the peculiar refraction of light by the thickened membrane.)—(See Fig. 21, B.) The

Fig. 21.



Fig. 21. A.—Tubes in the pyramidal substance of the kidney, in the case of Campbell. (Observation XIV.) (40 diameters.) B. A similar section (as described above). (170 diameters.)

nuclei are obscured or invisible, owing to the thickness of the intervening wall, but nevertheless exist in considerable numbers. The Malpighian bodies and capillaries are usually obliterated. The kidney has in fact become, like the tumours whose structure it resembles, a true non-vascular texture.

The explanation of the peculiar extension of the pyramidal striæ towards the surface in these cases, is to be found in the fact, that even in the normal condition the convoluted tubuli have a general disposition from the bases of the pyramids towards the surface, in the direction of the striæ of the cones. This is evident from the facility with which the gland tears in that direction; although in the normal state this disposition is masked by that of the vessels, which, passing in straight lines through the cones, break into a complicated net-work of capillaries at the bases of the pyramids. In the present lesion, the vessels having disappeared, and the course of the tubes being strongly marked, their disposition towards the surface becomes manifest, and the abrupt line of demarcation between the cortical and pyramidal substance, caused by the presence of the vessels, is obliterated.

A very characteristic instance of this lesion is found in Observation XIII. It may be worth while to remark, that in this case the lesion above described occurred in common with bronchial dilatation, there being thus a precisely analagous state of the pulmonary and renal texture. In the following case there are several unusual circumstances—the amount of exudation—the persistence of some of the vascularity of the surface and pyramids—and the cysts of the Malpighian bodies.

OBSERVATION XIV.—*Waxy Kidney (last stage, with Granular Exudation). Cysts in Malpighian Capsules.*—Agnes Campbell, æt. thirty, was admitted into the Royal Infirmary, February 8, 1848, under the care of Dr George Paterson. She laboured under great general anasarca, and was extremely anemic. There was a murmur with the first sound of the heart. The urine was found to be highly albuminous, specific gravity 1·012, acid reaction. She died February 20th.

On dissection, the external aspect was excessively anemic; the face was puffy, the legs oedematous. The cavities of the chest and abdomen contained a considerable quantity of fluid. All the organs were sound, except the kidneys. These were enlarged by about a fourth, and were firm and dense. The capsule was easily stripped; the surface not uneven; the venous vascularity less than usual, and irregularly distributed. The cortical substance generally was of a yellowish colour, with some opacity, and destitute of vascularity. The yellowish opaque colour penetrated between the tubular striæ towards the bases of the pyramids, which were broad, and displayed a good deal of vascular injection. The Malpighian bodies were prominent on the surface of the section, and presented an unusually transparent and pale appearance.

On examining microscopically a section by Valentin's knife, there was seen dispersed among the tubes of the cortical substance a considerable quantity of very finely granular exudation. A few cells presenting the appearance of the granular or exudation corpuscles were seen. The nuclei in the tubuli were mostly free from cell-walls. There were numerous fusiform cells and rudimentary fibres. The Malpighian capsules were thickened, and no blood-vessels could be seen in their interior, which was divided into loculi.

On separating the Malpighian bodies from the capsules (which was readily done by scraping the cortical substance), and viewing them either with strong or weak powers, they were seen to be composed of pale semi-transparent cysts, from 1-15th to 1-12th of a millimetre in diameter, and compressed together. When floated out, they presented the appearance of a cluster of grapes, except as regards the form of the individual cysts, which was circular. They were apparently retained together by a very slight medium, as by a little manipulation several of them could readily be separated from the mass. They appeared to contain transparent fluid.

The above transformation of the Malpighian bodies has not escaped the attention of Rayer, who says that in an advanced stage of the *Néphrite Albumineuse* "the glandules of Malpighi resemble small serous vesicles, mingled with others a little larger, which still later became true cysts."—(See Rayer's Atlas, Pl. IX. Figs. 6. and 7.) In the case just detailed, however, there was no enlargement of any of the Malpighian bodies.

CONCLUSION.

With the view of enabling the reader to place the foregoing observations in relation with the descriptions found in systematic pathological works, I subjoin the following short remarks on the principal physical characters usually ascribed to diseased kidneys.

Increase of Size and Weight—Hypertrophy.—Enlargement of the kidney occurs chiefly in consequence of three conditions; 1st, from sanguineous engorgement; 2d, from distension of the tubes by secretion or exudation; 3d, from permanent dilatation and thickening of the tubes. Of all these causes, the second is by far the most common. The last is characteristic of the waxy degeneration formerly described.

The quantity of liquid in the tubes is at all times subject to so much variation, that it is difficult to say what amount of increase of weight may be thereby occasioned without the existence of any positively morbid condition. It is not very uncommon to find kidneys otherwise not differing from the healthy standard, about double the usual weight, or between seven and eight ounces each. I have more than once found them to weigh nine ounces each, with very slight marks of disease. When the weight much exceeds this, it is probable it arises from the rare combination of vascular and tubular engorgement.

In kidneys containing oleo-albuminous exudation, the greatest increase of size is attained when the exudation is universal, and unaccompanied by desquamation.

Cystic degeneration of the kidneys, dilatation of the pelvis and ureters (Hydronephrose, Rayer), &c., also give rise to great increase of size and weight.

Diminution of Size and Weight—Atrophy.—This condition sometimes occurs to a certain extent in emaciated subjects, without any disorganization, owing to the diminished activity of secretion. More

frequently, however, it is the result of separation of the epithelium, followed by contraction and obliteration of the tubular structure.

Atrophy, from this cause, is liable to supervene in all other varieties of renal lesion, except the waxy degeneration, which appears to lead to a permanently hypertrophied condition of the organ. In kidneys enlarged from exudation, the occurrence of desquamation and its consequences is frequent; and the diminution of size in such cases, is often not followed by a return to the natural condition but by permanent atrophy.

The course of all disorganizing diseases in the kidney, is to produce first enlargement, and then contraction of the organ. In the extreme stages of the atrophy which results from exudation, exudation is often nearly absent. When exudation therefore, even in very sparing quantity, accompanies a contracted condition of the kidney, there is a probability that it has been abundant at some former period.

Irregularities of Surface—Tuberculated and Granulated Kidneys.—The smoothness of the surface in the kidney is destroyed either by unequal dilatation, or unequal contraction of the tubuli of the cortical substance. The former takes place in the waxy degeneration, the latter in the desquamative processes.

The most frequent irregularities of surface are formed in connection with the granulations of Bright (the origin of which is described, *May No.*, pp. 797-8). These are invariably formed when exudation is deposited in kidneys tending to the desquamative lesion; and, as this runs its usual course, the granulations become prominent from the destruction of the tubes around them. An extreme degree of the irregularities thus produced constitutes the tuberculated kidney.

The puckering and partial atrophy occasionally seen in kidneys otherwise not morbid, or comparatively slightly diseased, are probably in many instances the result of the obliteration of cysts.

The more remarkable changes in colour and consistence are described very fully in many parts of the preceding memoir.

On reviewing the whole of the observations, the result of which I have now laid before the public, I am induced to regard the following conclusions as especially important in relation to the pathology of renal diseases:—

1. By far the greater part of the pathological lesions of the kidney arise from, or are connected with, the exudation of oleo-albuminous granules into the interior of the tubes and epithelial cells.

2. The oleo-albuminous exudation is probably often preceded, and certainly occasionally accompanied, by vascular congestion; but when the quantity of exudation is considerable, more or less complete depletion of the vascular system invariably occurs. This is a secondary result of the obstruction of the *tubuli uriniferi*.

3. The oleo-albuminous exudation occurs in two chief forms; viz. *first*, Universal infiltration of the tubes throughout the organ;

and *second*, Infiltration of particular sets of tubules, the rest remaining free, or nearly so. In the latter mode arise the granulations of Bright.

4. There is no essential anatomical difference between the exudations in the kidney which are the result of chronic processes, and those which have been considered as the result of inflammation.

5. The capillary vessels of the kidney are subject to spontaneous obliteration (unaccompanied in the first instance by any visible lesion of the tubes), giving rise to the peculiar affection which I have called the *waxy degeneration*. This obliteration of the vessels is probably in all cases preceded by a stage of congestion.

6. The consequence of the waxy degeneration is thickening and varicose dilatation of the tubuli throughout the organ.

7. The tubes of the kidney are subject to contraction and obliteration, in consequence of the desquamation of their epithelium; a condition resulting in atrophy, and complete disorganization of the organ.

8. The desquamation of the epithelium occurs very frequently in all the other diseased conditions of the kidney. When sufficiently long-continued and extensive, it produces contraction, and this indifferently whether exudation be present or not. It is sometimes accompanied by vascular congestion in every stage of its progress.

9. The earlier stages of the exudations can only be discovered by means of the microscope. The progress of the waxy degeneration, on the contrary, is best traced by the unaided eye. The desquamation of the epithelium is only to be discovered with certainty by means of the microscope, and is particularly apt to escape attention, under all circumstances, if the *kidney* only, and not the *urine*, be looked to. It results that careful investigation, both by the microscope and the naked eye, both of the kidney after death and the urine during life, are indispensable to enable the pathologist to determine with exactitude the presence or absence of disease.

[I propose to publish the results of my observations on the urine, and on the pathology of Bright's disease, and other diseases of the kidney, as soon as some investigations suggested by the present series of papers are completed.]

ARTICLE IV.—*A Case of Feigned Insanity, with some farther Remarks on that Subject.* By C. LOCKHART ROBERTSON, M.D., Army Medical Staff; attached to the Royal Military Lunatic Asylum at Yarmouth; consulting Physician to the Cumberland Lunatic Asylum at Dunston Lodge.

In a few notes on the subject of feigned insanity and its diagnosis which I recently brought to the notice of the profession,¹ I made the

¹ Notes on Feigned Insanity. The Journal of Psychological Medicine, No. II. April 1848, p. 227, et seq.

following remarks on the case I am now about to detail:—"I have at present a case under my observation, in which I suspect this variety of mental disease (*chronic mania*) has been feigned with the above object (*viz. for the purpose of obtaining a discharge from her Majesty's service*). The patient at present professes to be cured of the insanity which he stated he laboured under on admission into this asylum. At that period he presented symptoms of chronic mania, talking unconnectedly, but only when he considered himself to be observed; while he exhibited a loss of memory not reconcilable with the recent invasion of the disorder or with its extent. The threat of coercive measures sufficed to produce in a few days what he terms his cure. The existence, however, of a depression in the left frontal bone—the result of a kick from a horse—together with one or two minor symptoms of mental derangement, demand a longer period of observation¹ before any decided opinion can be expressed, and aid in the illustration of the difficulty of diagnosing feigned partial insanity." The principles which guided me in confirming my diagnosis, will probably in the first instance be best illustrated by a detailed history of the case.

A CASE IN WHICH THE SYMPTOMS OF CHRONIC MANIA WERE SIMULATED, APPARENTLY WITH THE VIEW OF OBTAINING A DISCHARGE FROM HER MAJESTY'S SERVICE.

Pte. ———; her Majesty's ——— Regiment; aged twenty-two; eleven months since, was admitted into the Military Lunatic Asylum on the 26th November 1847.

From his history, it would appear that, in June 1847, on being discharged from the Regimental Hospital at ———, instead of joining his depot there, he embarked in a steamer for ———, then the headquarters of the regiment. On his arrival there he was taken to the guard-room, doubts being entertained of his sanity; and was subsequently transferred to the Regimental Hospital at that station.

On the 24th of June he made his escape in the Hospital dress,² in the which, however, he was that same day retaken. The reason he assigned for having taken this step was, to "escape from a man who was going to knock his brains out with a bar of iron." His conversation at this period was rational, and he talked a good deal about his own affairs, but obstinately refused to obey any order. He complained much of headach, but there was no symptom of any physical disorder; he slept and ate well, &c. He continued in the Hospital until the 14th of September, refusing to go out of doors for exercise, assigning as a reason, "that he was sure of being attacked by the stranger, who was waiting outside to knock his brains out."

On the morning of the 14th September he went into the ward adjoining that in which he was lodged, in which there was but one patient whose regimentals had not been removed on his admission that morning, as is the practice. The ward was at that moment empty. He immediately dressed himself in the regimentals, slipped out of the Hospital, and passed the sentry at the gate, holding a handkerchief to his jaw, as if he had just been having a tooth extracted. Half an hour elapsed before he was missed, and no tidings were heard of him until he was taken by the civil powers as a deserter, and lodged in the jail of ———, from whence, in consequence of his violence and presumed insanity, he was removed to this establishment.

¹ This essay, although only published in April, was written in February.

² A blue flannel dressing-gown, trousers, and waistcoat.

On admission, his manner was calm and collected. His bodily health appeared to have been injured by confinement. On the right side of the os frontis a depression was observed, which he stated he had received by a fall from a horse some years previous to enlistment.

His conversation on parade the morning after his admission was very unconnected. He insisted that he was in a jail, and "that the other patients were either drunk or pretending to be drunk, but not mad," &c. &c. Doubts being, from the previous history of his case, entertained of his insanity, I told him that, if he again repeated such absurdities, he should have a cold shower bath, and be secluded for the rest of the day. *He never again spoke in this incoherent manner during the entire period of his residence here.* He obeyed every order that was given to him, and invariably conducted himself in a most soldierlike manner.

On being questioned as to his former conduct, his desertion, &c., *he invariably denied all knowledge of the matter; of his ever having been in a civil jail, &c.* He likewise professed to have forgot the circumstances attending his arrival here, and exhibited an incoherence of thought and loss of memory in relation to these matters, inconsistent with the recent origin and slight symptoms of his supposed mental disease, as also with the otherwise perfect soundness of mind, which, from the period of his admission, he invariably presented on examination, excepting on the morning after his arrival, as already stated.

He continued apparently of sound mind, satisfied with his position, and employed daily with the working party until the 5th of February, when he addressed the following letter to his uncle:—

LETTER I.¹—Yarmouth, February 5, 1848.—MY DEAR UNCLE,—I take the opportunity of writing you this letter, in hopes to find you and your wife enjoying good health, and all friends, as I am at present, I thank the Almighty God.² Dear uncle, I daresay you will be very much surprised, if not before now, by hearing of me being a patient in the military madhouse; but I thank God I am quite recovered this last month or two. I have been labouring under this dreadful malady this last eight or nine months; but I shall ever return the Almighty God thanks that I am quite recovered at present, and I hope I will continue so, which I have no doubt, with God's help, that I will. Dear uncle, I am permitted by the gentlemen of this establishment to write this letter to you, in hopes that you will send for me. *I expected a short time after my recovery to be sent back to my depot, but it appears quite the reverse, according to what I hear.* They will never entrust me nor any other man with fire-arms, for fear of a return of that dreadful malady; but they are glad to get shot of them as soon as they recover. The government would not give twopence for me, nor a ship-load like me.

First, write a letter to the Secretary at War, and another to the gentlemen of this establishment, stating to those gentlemen that I am your nephew, and if they will be so kind as to send me to you, that you will hold yourself responsible; if I am not able to take care of myself, that you will see me taken care of. [*These are exactly the steps which relatives require to adopt who wish to remove convalescent or harmless patients from the establishment.*]

My dear uncle, let this not frighten you. You may depend upon it; if I thought that I would be a burden on you, or any of my relations in particular, I would deny that I had any, and then I would be sent to the parish I was born in, when the gentlemen of the establishment would think proper. I suffer very much by times with the original complaint, that pain in the head. You see that insanity has rendered me unfit for the army, and I am sorry of it; but welcome be the will of God. There are officers, non-commissioned officers, and privates, patients in this establishment. Some of them are here

¹ This and the two following letters are altered as regards the orthography; otherwise they are printed verbatim as written.

² The patient is an Irishman.

these number of years, and never get properly well, and the gentlemen doctors would not send them away under any consideration. They are very comfortably situated here, and so am I too; but, as a matter of course, I would much rather be out of it. I am no good for Queen or country here. When I am discharged, I will look out for some humble situation. I never will work at my trade any more, for fear of that other complaint returning. I don't forget my poor mother. On the 7th of this month she will be dead twelve months. That will be next Monday. I hope she is better off than to be in the world, poor woman. Dear uncle, my feelings have been overcome writing these few lines. Death is a debt we all must pay, and we do not know the moment. I will conclude at present. Your affectionate nephew.

On reading this letter, I informed him that he was entirely mistaken as to the facility with which he appeared to think he could be discharged from the service; and I assured him, that as soon as Dr Sillery considered him recovered, he would be reported as fit for duty and sent to the headquarters of his regiment. With this prospect he expressed himself extremely satisfied.

He continued apparently in a sound state of mental and bodily health, daily employed with the working party until the 8th of March, when he handed in the following letter, addressed to the medical officers of this establishment.

LETTER II.—On second recollection, Dr Sillery and Dr Robertson, gentlemen, I write those few lines to inform you both, and you both may inform any person or persons you think proper, I, lance-corporal ———, ——— regiment, it is quite evident since my recovery that I have both wished and asked to be sent back to my depot, and now my mind is quite to the reverse. I am certain sure I have not the least idea whether I will be sent back or not; but if I am to go back to my depot, I would certainly not wish to do so on any account. I will now state my reason, *that if I got a return of that dreadful malady, I might perhaps shoot myself or run the bayonet through my carcase, and then there would be no more of me; but then, on the other hand, it would be as likely I would do so to some of my comrade soldiers or officers, and that I would be made a prisoner of directly. But they would soon find out that I was once the patient in the lunatic asylum, and then what would they do or say but that I got a return of my former complaint?* I would be sent to hospital directly, where, I suppose, my head would be shaved and blistered. Do not think for a moment, gentlemen, that I have any particular wish for my discharge. Quite to the contrary. I have not; but I would prefer it before I would go back to my depot. But, whether I go back or not, I would certainly wish to go out of this. I thank the Almighty God I am better fit to be an orderly to take care of the patients, than to be a patient and the orderlies to take care of me. I hope, gentlemen, that you will take it into consideration, and not delay me more than another week or so, whether I am to be discharged or sent back to my depot.—I am, your most obedient servant, ———.

From the date of the above letter (March 8th) up to the 10th of May, he continued in good health, apparently in every way of sound mind, and daily employed on the grounds of the establishment. On that day, he handed in the following letter, addressed to the Right Honourable the Secretary at War.

LETTER III.—Sir, this is the humble petition of ———, ——— regiment, to you the Right Honourable the Secretary at War. I have taken it on myself to make known to you my grievances, as follows:—I was sent here last November, then labouring under insanity, but it pleased the Almighty God I was not long here when I was restored to my senses. As my former complaint renders me unfit for further service, I hope my body will not be detained much longer. I can assure you, Sir, it is most heart-rending to a man like me, that is in his perfect senses, to be deprived of his liberty. Sir, I am very well aware there could not be a more comfortable dwelling for men that is not capable of taking care of themselves than this establishment is. I hope, Sir, you will take

it into consideration, and not keep me much longer in bondage.—Your humble obedient servant, —————.

On the 13th of May, while working in the front square, he attempted to make his escape, but was immediately observed and brought back. On being reprimanded next morning, he stated to me that he was in his perfect senses, and ought not to be any longer detained in an hospital for the insane. I told him that, in my opinion, there never had been much the matter with him, and that I considered he had assumed the symptoms of mental disease with a view of obtaining his discharge. He said, "That if he had not been insane, the medical officer at the jail ought not to have sent him to a madhouse, and that I would find my view of his case difficult to prove."

He was ordered again to join the working party.

On the morning of the 16th of the same month, he was found to have escaped during the night, and, notwithstanding the most diligent search, up to the present date (July 1), no tidings have been heard of him.¹

The gradual development which the diagnosis of this case underwent under careful and continued observation, confirms the value of the statement, that "in cases of difficulty, confinement with observation in an asylum for the insane, is our last resource, and cannot fail in the course of time in proving successful."² The doubts as to the existence of mental disease, which the incoherence of thought and loss of memory *on one subject alone* raised, were more and more confirmed by the patient's own letters and conduct, until the proof of his malingering became so clear, that he himself could not deny the truth of the inference.

Again, the influence the emotions of fear and of hope exerted over him prove the value of the diagnostic—I, on a former occasion, laid down, "that as such persons (those labouring under chronic mania) are insensible to the operations of the passions of hope, fear, anger, &c., the emotions of these may, in those feigning this disorder, perhaps be produced."³ The threat of a shower bath and seclusion removed for good all symptoms of incoherence of thought in general conversation; the assurance, that on recovery he would be sent back to his duty and not discharged the service, as he in Letter I. anticipated, led to the writing of Letter II., which, in the hint of future violence, and in the statement, that if he were sent back to duty he might "shoot or run the bayonet through some of his comrade soldiers or officers," &c. &c., contained farther confirmation of the feigned character of his disease, inasmuch as they evinced in the patient's mind a sensibility to the emotions of hope, fear, and disap-

¹ The manner in which the patient effected his escape displayed great forethought and ingenuity. The details would, however, hardly be appreciated by those unacquainted with the building and locality.

² Notes on feigned insanity, already quoted.

³ Shakspeare, who evidently must have studied insanity from nature, notices this in that beautiful delineation of feigned dementia or chronic mania, in the character of Edgar:—

"My tears begin to take his part too much,
They'll mar my counterfeiting."—*King Lear*.

pointment, which, had the disorder he assumed been real, would not have existed.

Lastly, all doubts as to the nature of the case were entirely removed by his assertion on the 18th of May, when told that he was feigning insanity, "that it would be difficult to prove it, and that the medical officer of the jail ought not in that case to have sent him to a mad-house;" while his subsequent escape, and its attendant precautions, confirmed more decidedly than was agreeable the fact, that the patient had simulated mental disease with the view of obtaining his discharge from her Majesty's service.

In conclusion, I think that the following remarks—the only observations on this subject which I have met with in our systematic treatises on insanity, and which I had not seen when I published the "Notes on Feigned Insanity" I have in this paper more than once alluded to—may be interesting in their relation to the case under consideration, and serve also as a sample of German psychology:—

"The detection of simulated (feigned) psychopathies," says Baron Feuchterleben,¹ "is likewise attended with great difficulties. The two objective somatically characteristic symptoms which Friedrich² mentions as criteria of the psychoses, namely, the specific smell and the peculiar physiognomy, are by no means always present. The physiognomy may, moreover, be counterfeited so as to deceive. Even the sleeplessness, generally (but not always) connected with psychopathies, may be assumed (though hardly beyond a certain point). The reluctance to look you in the face is not to be depended upon, because it also occurs in lunatics.³ In all cases a knowledge of the object of the supposed simulation is of much importance, because it points out a motive through which (after many a contrivance to be invented according to the individual case) the mind of the supposed simulator may be acted upon. Moria and idiocy are more frequently, mania more rarely simulated. In pretended monomania the existence or non-existence of hereditary disposition, of physical symptoms, and of other (ethical) motives, for the imputed crime, in connexion with a comparison of the deed with the character of the individual, which must be inquired into in all attainable particulars, may serve as guides to the decision of the physician; wherein it is true, as in every decision, no small scope must be left to the judgment for a free exercise of its powers. Hypocrites in general adopt conduct diametrically opposed to their natural character; the cunning pretend to be stupid; the cheerful, melancholy; the gentle, furious,⁴ &c. Let the physician, therefore, set out for this point; let him accurately examine the harmony or want of harmony in the psychical and physical qualities of the individual; let him inquire long, surprise, deceive, tire the feigner; let him employ disagreeable psychical and physical means or alleged remedies, with women those which may disfigure them, and in the most obscure cases let him wait, with an indifference which is beyond measure painful to the simulator, the effects of time, the solver and elucidator

¹ The Principles of Medical Psychology, being the Outlines of a Course of Lectures, &c. Translated from the German, by the late H. Evans Lloyd, Esq., and revised and edited by B. G. Babington, M.D. For the Sydenham Society. Pp. 376 and 377. London: 1847.

² System der Gerichtlichen Psychologie, p. 162.

³ Wagner Ger. Arzneik. Vol. II. p. 158.

⁴ Sinowwitz Geistes Störungen, p. 468.

of all things. It must not be forgotten, however, that men have really become mad after having been long confined on suspicion of pretending to be so, just as hysterical women often really fall into a state which they have long affected."

Part Second.

REVIEWS.

Traité de l'Art de Formuler, ou Notions de Pharmacologie appliquée à la Médecine. Par M. MIALHE, M.D., Pharmacien, Professeur agrégé à la Faculté de Médecine. Paris: 1845. 8vo. Pp. 518.

A Treatise on the Art of Prescribing. By M. MIALHE, M.D., &c. Paris: 1845.—(Continued from p. 43.)

In a former article upon this treatise, after a brief survey of the author's views on the general absorption of medicines, we shortly adverted, in detail, to two important drugs which are discussed by him, namely, sulphur and iron. The subjects of antimony, silver, and mercury, will now engage our attention.

And first, as to *antimony*. As this metal undergoes oxidation when exposed to the combined influence of air and moisture, there is no difficulty in understanding its action when taken into the stomach in the metallic form. Further, as the oxide thus generated is, according to our author, in the state of hydrate, it is dissolved in a feebly acid menstruum, as the gastric juice, much more readily than the ordinary sesqui-oxide of antimony, more or less of which is probably anhydrous. Having digested equal parts of the pure metal in a state of fine division, and of the sesqui-oxide in separate portions of water very feebly acidulated, Mialhe ascertained that the quantity of antimonial salt produced with the metal was much greater than with the oxide, from which he infers that the activity of an antimonial preparation, of which the oxide forms the base, will be much increased when the latter is in the state of hydrate. It is not mentioned how the oxide used in this experiment was prepared. Gmelin (*Handbuch der Chemie*, 1844) says, that the sesqui-oxide of antimony does not appear to form a hydrate, but, as Berzelius has shown, it is to a slight extent soluble in water. The sesqui-oxide of antimony belongs to a small class of drugs which are soluble alike in the acids of the gastric juice as in the alkalis of the intestinal canal, comporting itself as a base in the one case, and as an acid in the other; but for easy solution in feebly acid or alkaline fluids like the gastric and intestinal secretions, Mialhe affirms that it must be hydrated.

Of late years in France, the kermes mineral has again come into vogue as an antimonial preparation, and, owing to the recommendations of Clusel, Trousseau, and others, has with many superseded the use of tartar-emetic. It is said that, while in its general therapeutic action it is quite equal to the double tartrate, it is much less apt to excite vomiting, local irritation, or inflammation. This compound seems worthy of a further trial in this country; but the circumstance to which we wish to direct attention here, is the great difference in its strength, according as it is obtained by the moist method of Clusel (official in the Parisian codex), when it is most active, or by the dry method of Thierry, when it is often nearly inert. Our author has ascertained, by experiment, that the official kermes contains a much larger proportion than the other of the hydrated oxide. May not the very irregular action of the sesquioxide itself, observed in the trials made with it by Professor Christison and Dr Clark, be owing to its containing a variable proportion of hydrated oxide?

The following explanation is offered by Mialhe, of the remarkable difference in the physiological effects of the same doses of tartar-emetic in health and disease. In the healthy stomach the tartrate is, in part, immediately decomposed by the free muriatic acid of the gastric juice, and a highly acrid compound, the hydro-chlorate of the chloride of antimony, is formed. Hence the nausea, vomiting, and diarrhoea. In the course of an acute disease, as pneumonia or bronchitis, in consequence of the loss of appetite and abstinence from food, the gastric juice becomes very feebly acid, and frequently indeed presents no acid reaction whatever. There is little or no formation of the acrid chloride, while the passage of all the tartrate into the circulation, fully accounts for its powerfully sedative or contro-stimulant action under these circumstances. Mialhe endeavours to account for the well-known fact, that vomiting which has been excited by small doses of tartar emetic, frequently disappears when the dose is increased, by supposing that an amount of hydro-chloric acid in the gastric juice, capable of decomposing a quarter or half a grain of the medicine, may be insufficient to displace the tartaric acid when the quantity is larger. To these views it may be objected, that as the emetic action of antimony is obtained by injecting it into the veins or rectum, the vomiting should be regarded as the effect rather of a special modification of the nervous system, than of a local irritation of the mucous membrane of the stomach; and a still stronger objection is found in the results of several experiments in which we administered, along with one grain of tartar emetic, thirty grains of calcined magnesia or prepared chalk, without interfering in any degree with its ordinary emetic action. The antacid was not given in one, but in three separate doses; the first at the same time with the antimony, the second a quarter of an hour afterwards, and the third in half an hour.

While we reject M. Mialhe's explanation, we are not the less opposed to that view which attributes the *tolerance* to the presence of inflammatory fever. In fact, the same tolerance is established, *quite independently of acute febrile reaction*, when the patient is confined to a spare diet; and it may be established as a general rule, that the more rigid the diet, so much the more marked will be the general action of antimony; and, on the contrary, when the quantity of food is considerable, its emetic action is more decided.

Silver.—The insolubility of this metal in the gastric juice renders it inert in relation to the animal economy. The silver of the nitrate, on meeting with that fluid, is immediately thrown down as the insoluble chloride. The complete absence of any sensible effect, in the great majority of cases, during the administration of the nitrate of silver, has led many (we refer more especially to the German) physicians to deny the occurrence of its absorption in any case, and to suppose that the entire dose escapes in the form of chloride with the *feces*; but the coloration of the skin, which has followed its use, though only in a few instances, places the fact of its absorption, at least in these cases, beyond a doubt; and this is made intelligible by Mialhe, who affirms that the chloride is soluble to a slight extent in a solution of common salt or sal ammoniac, both of which are present in the gastric juice. The chloride of silver, freshly precipitated in the stomach, is in a more favourable condition for solution than the same preparation in the dry form; hence the superiority of the nitrate of silver to the dry chloride as a therapeutic agent. But how is the rare occurrence of the coloration of the skin to be explained, unless perhaps we suppose, that in those unfortunate individuals in whom it has taken place, the fluids were, from accidental circumstances at the time of administration, unusually saline? As to the oxide, there is little doubt that it also is converted into chloride in the stomach, in which case it must be equally liable with the nitrate to produce the bluish-grey tint of the integuments. The following formula is constructed in accordance with the views above mentioned:—

Nitrate of Silver Pills.

Nitrate of Silver,	-	-	-	-	15 Grains.
Chloride of Sodium,	-	-	-	-	60 "
Starch,	-	-	-	-	45 "
Gum Arabic,	-	-	-	-	15 "
Water,	-	-	-	-	q. s.

Pulverize the nitrate of silver; then add the water, and afterwards the salt; lastly, the starch and gum. Divide into 100 pills; each pill contains a little more than the sixth of a grain.

As the quantity of chloride which is rendered active, even under the most favourable circumstances, is very small, it is evident that, when silver is exhibited to procure its *remote effects on the nervous sys-*

tem, a preparation which is at the same time soluble, and incapable of decomposition in the gastric juice, is much wanted. We hope to have this desideratum supplied by a solution of the chloride in the hypo-sulphite of soda. This preparation has a saccharo-metallic taste, and a dose of it, containing an amount of silver equivalent to two grains of the nitrate, is easily borne by the stomach. As it is not decomposed by *diluted* muriatic acid, or solutions of the phosphate of soda, muriate of soda, and muriate of ammonia, there is reason to believe that it is not acted upon by the gastric juice, but passes directly into the circulation. The mode of preparing this solution, together with its physiological and therapeutic action, we shall notice on a future occasion.

We shall close our remarks with an analysis of the author's observations on *mercury* and its compounds.

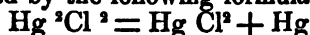
M. Mialhe has demonstrated by an extensive series of experiments, that all the preparations of this metal used in medicine, in reacting with a solution of an alkaline chloride, with or without the presence of atmospheric air, produce a certain quantity of corrosive sublimate. The amount of bi-chloride thus formed differs considerably, according to the nature of the mercurial preparation employed. The bin-oxide of mercury, the greater part of the binary compounds which correspond to it in composition, and in general all the per-salts of mercury, in contact with an alkaline chloride, give by double decomposition bi-chloride of mercury and a new alkaline salt. The protoxide, and the greater part of the binary compounds which correspond to it in composition, are converted first into calomel, and it is only by a subsequent reaction that a very small proportion of corrosive sublimate is formed. These reactions take place at ordinary temperatures, but are much promoted by a heat equal to that of the human body. Some are affected immediately, and the greater number do not require more than a few hours.

For the reader's convenience, we have thrown into the tabular form the results obtained in experiments with the more important mercurial compounds. In each experiment, six decigrammes of the mercurial preparation were placed in contact with an alkaline liquor having the following composition:—common salt, sal-ammoniac, of each, six decig.; distilled water, ten grammes. The operation was continued in all the experiments for twenty-four hours.

The process adopted by Mialhe to estimate the quantity of bi-chloride formed, is, we believe, quite exact. It is a modification of that recommended by Gay-Lussac.

MERCURIAL PREPARATION EMPLOYED.	QUANTITY OF CORROSIVE SUBLIMATE FORMED.	
	At the Ordinary Temperature, from 15° to 20° Centig.	At a Temperature of 40° to 50° Centig.
Calomel (prepared by sublimation.)	6 milligrammes ¹	15 milligrammes.
Proto-bromide,	6 "	15 "
Protiodide,	5 "	6 "
Protoxide,	11 "	19 "
Proto-nitrate,	4 "	13 "
Proto-sulphate,	7 "	14 "
Proto-acetate,	8 "	11 "
Proto-tartrate,	4 "	8 "
Ammonio-chloride,	82 "	180 "
Bin-oxide,	47 "	154 "
Bin-iodide,	110 "	193 "
Bi-cyanide,	Entirely converted into bi-chloride.	
Per-nitrate,		
Per-sulphate,		
Turbith Mineral,	112 "	228 "
Bi-tartrate,	312 "	362 "
Metallic Mercury,	4 "	7 "
Per-sulphuret,	Are not acted upon when pure.	
Black Sulphuret,		

So long ago as 1763 Capelle affirmed, that calomel, when mixed with muriate of ammonia in solution, undergoes some important change; but as to the nature of the chemical reaction thus induced, chemists were by no means agreed. Some maintained that the proto-chloride was merely held in solution; others, that a portion of it was converted into corrosive sublimate and metallic mercury, the truth of which view Mialhe has been the first to demonstrate by experiment.—(P. 10, *et seq.*) When the experiment is performed with distilled water, and the atmosphere is excluded, the change is correctly represented by the following formula:—



But the reaction is more complex when the fluid is exposed to the atmosphere, in which case the quantity of sublimate produced is much greater. Under these circumstances, the calomel absorbs a certain quantity of oxygen, one equivalent of bi-chloride being produced for every two equivalents of oxygen absorbed; further, each equivalent of the bin-oxide of mercury thus formed gives, by double decomposition with the chloride of sodium or ammonium in solution, one equivalent of sublimate and two of soda or ammonia.

In the case of calomel, Mialhe has performed a number of ex-

¹ 100 milligs. = 1 decigramme.

1 decig. = 1.54 gr. English Troy.

periments to determine the effect of circumstances in modifying the amount of bi-chloride formed. He has thus shown :—

1. That when calomel, prepared by precipitation, is employed, the quantity of bi-chloride produced is greater than from the same drug obtained by way of sublimation.

2. That the quantity of bi-chloride produced bears no proportion to the amount of calomel employed ; but is in exact relation to the quantity of alkaline chloride present in the solution.

3. That the quantity of sublimate produced is greatest when the alkaline solution is most concentrated.

4. That the presence of organic matter in the mixture does not prevent the reaction, and that while fatty matter retards, dextrine favours it.

5. That although the change may take place in water entirely deprived of air, yet it is promoted in a marked degree by exposure to the atmosphere, the quantity of bi-chloride formed being more than three times greater in the latter than in the former case.

The immediate agent of the physiological action of all mercurial compounds is, according to Mialhe, the corrosive sublimate formed by the agency of the gastro-intestinal fluids, which contain the muriates of soda and ammonia, accompanied or not with muriatic and other acids, which promote the reaction on the preparation employed ; and, as a corollary to this proposition, he maintains that their activity is in exact relation to the facility with which they are converted into bi-chloride. However true this proposition may be in reference to the insoluble mercurials, we deny its application to all the soluble compounds. We have induced salivation by the use of *large* doses of a soluble and non-irritant salt, the tartrate of potash and mercury, without exciting the slightest irritation of the stomach or intestines ; for which, and other reasons derived from the chemical properties of this salt, that we cannot now enter into, we presume that it is absorbed directly, without previous conversion into the acrid bi-chloride.

By inspecting the table which we have given above, the reader will observe, that it corresponds in its results pretty closely with clinical experience ; thus, the bi-cyanide and per-sulphate of mercury, which are known to be heroic poisons, are there seen to be entirely converted into corrosive sublimate ; the results obtained in the experiments with the ammonio-chloride, bin-oxide, and bin-iodide, tally satisfactorily with their well-known energy ; while the small amount of bi-chloride formed from the proto-chloride, proto-bromide, and proto-acetate, accounts for the lesser activity with which these agents are endowed. At the same time, we think that the proto-salts hold here a lower place than experience has generally assigned to them. That the alkaline chlorides and muriatic acid should have no action on cinnabar, or the black sulphuret of mercury, is in exact accordance with the inert character of these compounds.

The great difference in the quantity of corrosive sublimate form-

ed from the proto-salts, when compared with the per-salts, is well shown in the iodide and bin-iodide, proto-tartrate and per-tartrate. Mialhe affirms, that the physiological action of the last two salts differs in a like degree. The French possess two empirical preparations (liquors of Pressavin and Diener,) which contain the tartrate of mercury for their active ingredient. These are very unequal in activity; the preparation which at one time displays an almost poisonous energy, at another acting as the mildest of mercurials. Mialhe has shown, that this is owing to differences in the mode of preparation, in consequence of which they contain, along with the proto-tartrate, very variable proportions of the per-tartrate.

Our author's experiments show, further, that the pure metal itself, when exposed to the action of the atmosphere and of alkaline chlorides in solution, is converted in part into corrosive sublimate; and to this fact we can have little hesitation in ascribing the therapeutic action of metallic mercury, and of the compounds in which it is present in a state of fine division. When these contain in addition the protoxide, their activity is augmented, as the table indicates; but that their action, as mercurials, is due solely to the small proportion of oxide which may be present, cannot now be maintained. The pure metal, even when given in the fluid form, is not inert. As administered generally, it passes so rapidly through the intestines, that no time is afforded for the gastro-intestinal fluids to act upon it. But if from any cause it be retained, the result is very different; as in the case of a woman recorded by Mialhe, to whom fifteen ounces of the pure metal were administered to relieve obstruction of the bowels. She lived for six days afterwards, during which period the symptoms of obstruction continued, and the mercury was retained in the body. M. Mialhe saw her on the day of her death; the pulse was then almost imperceptible, the skin was cold, and the face was emaciated and painfully contracted. She complained of acute pain in the belly, which was enormously swollen. But in addition to these symptoms, which might be due as much to the intestinal affection as to the mercurial poisoning, she was affected with a peculiar and constant trembling of the superior extremities, and of the lower jaw; the gums were violet-coloured and bleeding; the inferior incisors had all fallen out two days previously; one only of the superior incisors remained, and it was so loose that the slightest effort would have sufficed to extract it; the inferior maxillary bone was exposed in several points over the areolar processes, and the mouth exhaled a strong mercurial and fetid odour. There was little salivation. A post-mortem examination was refused.—P. 107.

This view is opposed to that of Dr Christison, who doubts whether metallic mercury, swallowed in the fluid state, has ever excited mercurial action, and who believes that all the mechanical preparations of this drug probably contain a quantity of oxide sufficient to

account for their specific effects. The researches of Mialhe, in reference to arsenic, mercury, and other metals, fully establish the correctness of the general law in physiology laid down many years ago by Dr Christison, "that metals do not act as such, but must first be converted into oxides or salts" (*Dispensatory*, 1st Ed. p. 503); but they show further, that the changes which are requisite for absorption may take place in the interior of the body.

There are many circumstances in connexion with the physiological action of calomel, which admit of explanation on this theory, which attributes its absorption to a partial conversion into corrosive sublimate; such as the equal activity, in so far as absorption is concerned, of small and large doses, the quantity of bi-chloride formed being determined by the saline strength of the secretions, and not by the amount of calomel taken; the advantage of the frequent administration of small doses, and in this manner exposing the chloride to the agency of a large quantity of the secretions. Ptyalism is said to be readily induced in seamen. Is this owing to an excess of chloride of sodium in their gastric and intestinal fluids, on account of their eating salted provisions? In cholera, and other enteric affections, we know that the blood is exhausted of its saline constituents, and that the secretions consist of little more than water. Now, placing this fact in connexion with the action of large doses of calomel in these diseases, in which, instead of exciting violent mercurialism, or "of adding to the local irritation, as might be expected from the well-known effects of that drug on the healthy bowels in less doses, they have a tendency to soothe pain, allay spasm, abate redness, and lessen excessive secretion" (*Christison's Dispensatory* p. 512), is it not probable that this remarkable suspension of the physiological action of the medicine, is dependent on the watery state of the secretions, and consequent non-production of corrosive sublimate? Unfortunately, this leaves the therapeutic action as far from comprehension as ever.

Having entered the circulation, our author supposes that the bi-chloride unites with the albumen of the blood; the combination thus formed, insoluble in water, is soluble when in the state of hydrate, in the solution of an alkaline chloride, as the serum of the blood. Of the existence of mercury in the blood, and its passage into the secretions, there is now no doubt. Mialhe succeeded in detecting it in solution in the urine twelve hours after taking a dose of nine grains of calomel. The quadruple compound of albumen, chlorine, mercury, and the alkali, which results, is very fluid and remarkably stable. It is not decomposed by hydro-sulphuric acid, the alkalis, or the ordinary tests of mercury, which fact, we think, offers a probable explanation of the difficulty of detecting mercury in the blood, and the general necessity for that purpose of destructive distillation. Is the formation of this compound, and its circulation with the blood throughout the body, the cause of that remarkable combination of symptoms termed mercurialism? The diminution

of the albumen effected in this way, would account for the dissolution of the blood, and loss of its plasticity, as indicated by paleness and hemorrhages, which is now generally recognised as an effect of mercurial action.

The general opinion that corrosive sublimate in combining with animal matters is reduced to calomel, is denied by Mialhe; and the results obtained in his experiments have been subsequently confirmed by MM. Chantourelle, Lassaigne, and Selmi. When, for therapeutic use, it is allied to certain organic bodies, as milk, sugar, or gum, its action is much milder, as it is thereby deprived of its irritant and caustic properties. Its absorption and general action on the system, are not the less certain, as was pointed out by Soubeiran in 1840.

There is one substance, however, contained in several pharmaceutical compositions, which has the property of converting corrosive sublimate into calomel. This is the formic acid, produced by the action of an alkali on glucose. Thus, when grape sugar is boiled with potassa, there is obtained a brown liquor, which has the property of immediately reducing corrosive sublimate to calomel, and lastly to metallic mercury. It is to the presence of this body that Mialhe attributes the diminished power of corrosive sublimate when added to certain compound syrups, and more especially to the compound syrup of sarsaparilla of the French codex, which is directed to be prepared by boiling. But it is not essential to combine the bi-chloride with organic matters to deprive it of its causticity. It is sufficient for this purpose to unite it with chloride of sodium or muriate of ammonia. After what has been said, it is unnecessary to add any thing in explanation of the *rationale* of the following formulæ :—

Solution of Corrosive Sublimate.

Distilled Water,	- - - - -	1000 parts.
Muriate of Ammonia, and Chloride of Sodium,	- - - - -	
of each,	- - - - -	2 parts.
Bi-chloride of Mercury,	- - - - -	1 part.

Dose, a table-spoonful three, four, or more times daily.

Pills of Corrosive Sublimate.

Corrosive Sublimate,	- - - - -	8 grains.
Chloride of Sodium,	- - - - -	30 "
Starch,	- - - - -	45 "
Gum Arabic,	- - - - -	15 "
Distilled Water,	- - - - -	q. s.

Divide into 48 pills; each contains one-sixth of a grain.

Ointment of Corrosive Sublimate.

Bi-chloride of Mercury,	- - - - -	4 parts.
Muriate of Ammonia,	- - - - -	8 "
Azunge,	- - - - -	30 "

This is an active mercurial preparation for external use, and ought to be used with circumspection.

From the foregoing outline of some of the subjects embraced in M. Mialhe's treatise, the reader will be able to form an opinion of the general character of the work. We cannot observe too much caution in accepting chemical explanations of physiological and therapeutic phenomena, more especially when coming from one who is not himself a practical physician; but although several of our author's inferences, as we have ourselves shown in one or two examples, should not be confirmed by future observations, still the facts on which they are based remain to science, and the erroneous deductions themselves will not be unproductive of good. Such inquiries are in an eminent degree suggestive; and though they may sometimes lead to error, yet, to use the words of Andral when speaking of the same subject, "L'esprit s'y arrête et y revient, comme s'il avait la conscience qu'elles le placent à un point de vue d'ou des vérités importantes vont lui apparaître."

There are portions of the work with regard to which the office of the critic might be exercised with some severity; more especially is the author open to censure when he leaves his proper science, which is that of pharmaceutical chemistry, and enters the domain of physiology. But the fault for which he is above all blame-worthy, is an obvious carelessness in distinguishing facts from mere suppositions, and actual observations from loose conjectures. These and other blemishes, however, we willingly overlook in consideration of the merits of the book, which is the fruit of much patient labour and superior ability. We are no more than just, when we characterise it as forming a most valuable contribution to our knowledge of the changes that drugs undergo in the stomach and intestines—a department of therapeutics, the importance of which can hardly be overrated, but which, it is much to be regretted, is still in its infancy.

Researches on the Motion of the Juices in the Animal Body. By JUSTUS LIEBIG, M.D., Professor of Chemistry in the University of Giessen. Edited by WILLIAM GREGORY, M.D., Professor of Chemistry in the University of Edinburgh. London: 1848. Pp. 109.

THE present work belongs to a class which we are always disposed to receive with pleasure, and which we never peruse without great expectations. We have always held, and have often asserted, in the pages of this Journal, that the true mode of cultivating the science of the vital phenonema is by giving scientific precision to our ideas of the relations of these with physical science; and we are convinced that it is only by a thorough understanding of the latter, that success can be attained in the attempt to reduce the more recondite and complex vital phenomena to those simple and general expressions which we term laws.

Physiological science appears to us to be at present in a transition state. The old parties have been broken up, the new are scarcely yet formed. The vital, chemical, and mathematical schools in physiology have each had their day, and have done their work; but all of these, considered as exclusive systems, may be said, with truth, to have vanished from modern physiology. If a physiologist of the present day speaks of a theory as being too mechanical, he refers to a particular instance; he does not mean to deny in general that mechanical laws enter deeply into the explanation of the phenomena of organized bodies; nor does a chemist attempt to refer *all* physiological phenomena to the laws of his own peculiar science. On the other hand, there are now few who would hesitate about admitting, that the result of chemical and physical reasonings, as applied to physiology, has been only the more clear and distinct isolation of phenomena to all appearance as different in nature from those of dead matter, as the phenomena of chemical affinity are from those of gravitation.

In building up chemical science, it was necessary to eliminate those phenomena which could be explained by the laws of mechanics. This done, the resulting phenomena were seen to be referrible, in great part at least, to a law of affinity which produced combinations of different substances in definite proportions, and with material alterations of their original most essential properties. In building up vital science a similar method must be pursued; the laws of physical and chemical science must be applied rigidly to the investigation of the phenomena of organized life; and thus by the method of exclusion we shall arrive at more definite ideas as to the nature of the laws which are peculiar to organized existence.

In this mode of inquiry Liebig has earned one of the greatest reputations of the present age. If some of his researches have exhibited, in particular cases, a too exclusively chemical bias, this is perhaps no more than was to have been expected; and no one can deny that, in his general scientific doctrines, Liebig has at all times showed the fullest and most correct appreciation of the present condition of physiological science. It is impossible, therefore, to turn to a new work of so distinguished an observer, without having our expectations immoderately raised. We must confess, that the present work does not entirely satisfy those expectations; still, it contains the result of an extensive series of researches, some of which will probably come to be of the greatest value in science, and the entire value of which will, perhaps, not be appreciated until they have been some time before the public. In this article, we propose to give our readers an idea of some of the principal results arrived at in this work, without, in the mean time, attempting to give a *systematic* view of the researches on which these results are founded. This attempt must remain to be made at a future period, if, indeed, that be possible for any one, which the author himself has not even attempted.

The title of this work is scarcely indicative of its exact nature. The greater part of the volume is devoted to an inquiry into the physical conditions of imbibition and endosmosis, phenomena which are by no means exclusively confined to, or illustrative of, the motions of the *animal* fluids, and which have been conclusively shown to occur in unorganized as well as organized textures. The body of the work is, therefore, a purely physical inquiry. Applications to animal physiology, and an account of Hales' observations on vegetables, occupy the latter part; but the former are illustrated by very few new observations, and are, indeed, more of the nature of speculations, thrown out for future inquiry, than of positive additions to physiological science. These hints, however, imperfect as they are, appear to us to be founded on sufficiently good data, and are certainly of the greatest interest in a physiological point of view.

With respect to the physical and experimental part of the inquiry, we find little which is not better and more philosophically illustrated in the lectures of Professor Matteucci of Pisa, delivered in 1844, and now so well known in this country; with which, however, Professor Liebig seems not to have been acquainted, as the name of their author is not, so far as we have observed, once mentioned in the present volume. This is the more to be regretted, as we cannot help thinking, that an acquaintance with the views and experiments of the Italian professor would have caused a considerable modification in those contained in the volume before us. They would, at least, have had the effect of stimulating the acute mind of Liebig to accomplish more than, as it appears to us, has been done in the present instance. We shall probably seize an early opportunity of bringing before our readers some of the recent contributions to this department of physical science. At present, we shall confine our remarks, for the most part, to a theory, which it appears to be one of the main objects of this series of researches to support, and in which Liebig decidedly differs from all other writers on the subject.

The general tendency of this work is, to claim for chemical attraction, or affinity, a much larger share than has been hitherto conceded to it in the production of the phenomena of solution, endosmose, imbibition, and even capillary attraction. In accordance with this view, the term "*chemical attraction*" undergoes, in Professor Liebig's hands, an extension of signification, of which it is necessary that our readers should be aware. Not only is a simple solution, or the intermixture of two liquids, such as alcohol and water, arbitrarily referred to the law of chemical affinity; but the simple circumstance of adhesion of the particles of a liquid to those of a solid, when the latter is moistened, is owing, according to Liebig, to an affinity between the particles of each. He admits, indeed, in the beginning of the work (p. 4), that "the penetration of a fluid into the pores of a porous body, is the result of capillary attraction;" but, throughout the work, he speaks of imbibition, and even moistening of bodies, as mainly the result of chemical attraction. Indeed, in p. 27, he boldly

asserts that chemical affinity is manifested *every where*, when two dissimilar bodies come into contact :—" Although we are accustomed to limit the notion of affinity to such cases as exhibit a change perceptible to our senses, in the properties of the substances employed, as, for example, when sulphuric acid and lime, or sulphuric acid and mercury, combine together, this limitation arises from the imperfect apprehension of the essence of a natural force."

According to Liebig, the solution of a salt in water is an example of chemical affinity ; the attraction of the atoms of salt for the water, being in this case stronger than their attraction for each other. In like manner, the precipitation of the salt from the water by the addition of alcohol, is likewise an example of chemical affinity, in which the former attraction is overcome by a yet stronger one, that of the alcohol for the water. Now if, instead of adding alcohol, and thus precipitating the salt from its solution, we place that solution in contact with a piece of dried animal membrane (or indeed almost any body capable of imbibing it), the salt is in this case also precipitated, provided the solution be sufficiently strong ; a result which, according to Liebig, springs from exactly the same cause as in the former instance,—viz. that the affinity of the salt for the water has been overcome by a stronger affinity, that of the membrane for the water ; and, as the membrane has little or no affinity for the particles of the salt, the latter is partially separated in the crystalline form.

Thus, according to this definition, a change in the properties of the substances (which has hitherto been identified with the very nature of chemical phenomena), is by no means necessary as a manifestation of affinity. Neither is combination a necessary part of Liebig's idea of affinity ; combination being only one result, which occurs when the attraction is stronger than all the obstacles opposed to its manifestation, and not otherwise.—(P. 27). Mere adhesion of particles, on the contrary, if they be particles of dissimilar bodies, such as water and membrane, water and clay, even water and glass, when the latter is moistened, is accepted as a proof that chemical affinity has been brought into play.

Now, we must say that this arbitrary alteration of verbal meanings appears to us a most illogical, and (where supported by so great a name) a most dangerous way of dealing with a scientific question. There is not, in the whole of the present volume, the shadow of a proof that the phenomena of imbibition and moistening of porous or other substances are due to any other force than that which has hitherto been well known under the names of attraction of cohesion, molecular or capillary attraction. There is not the slightest proof that the moistening of a porous body is in nature different from the moistening of any other body, or that the affinity which exists between the particles of water and glass, water and membrane, water and alcohol, is in any way different from that which undoubtedly exists between the particles of water and water, alcohol and alcohol, glass and glass, &c. Yet these last have been always admitted to

be the most undoubted instances of molecular attraction, a mechanical, not a chemical force.

That molecular attraction varies infinitely in amount, both between different substances, and in the same substance under different external conditions, is perfectly well known. That a porous substance, therefore, should absorb more of a fluid for which its attraction is great than of one for which its attraction is small, is not new either in theory or experiment. That water, by its attraction for salt, should reduce the particles of the latter to a molecular form, and diffuse them intimately through its substance, is another phenomenon of the same kind. That alcohol, by its greater attraction for the water, should remove the atoms of salt from their molecular condition, and resign them to their attraction for each other, is again due to a molecular attraction of the water for the alcohol, stronger than that of the water for the salt. Liebig has indeed admitted that capillary attraction is present in these cases; the only question is as to the presence of chemical affinity.

Now we maintain, that if molecular or capillary attraction, in the ordinary sense of the term, be sufficient to account for the phenomenon, it is unphilosophical to suppose chemical attraction superadded to it. If, on the other hand, it be asserted that capillary attraction and chemical affinity are in reality one and the same force, we reply that it may be so, but we do not find in the present work any additional evidence on the subject. The analogies between chemical affinity and molecular attraction must, we think, have struck every one, as well as the arguments which have been urged in favour of the identity of the latter force with gravitation. Nay, we are by no means insensible to the proofs which have lately been adduced, that the whole of the physical forces are essentially modifications of one great force acting universally throughout nature; but the evidence of such propositions as these must be drawn from extended observation, and new experiments upon these forces in every possible mode of their action, and not from a few crude analogies, which have been a hundred times observed. In the mean time it is customary and right for scientific purposes to distinguish between molecular and chemical attraction. Thus copper and silver may be intimately mixed, forming an alloy. This is molecular attraction; the combination takes place in all proportions indifferently, and the properties of the two substances remain the same as before, except in so far as altered by the physical law of mixture. Again, copper and silver each combine with oxygen. This is true chemical affinity, with combination in definite proportions, and is manifested by such an alteration in the properties of the compound as cannot be accounted for on simple physical laws. While we hold, therefore, as we do, the provinces of these separate classes of phenomena to be, in the present state of science, sufficiently well defined, we cannot but think it extremely unadvisable to adopt the terms and arguments of Liebig in the present work; for not only have they a tendency to confuse and

unsettle the mind of the inadvertent reader, but they appear to us to have led the author himself in many instances to a false application of the results of his own experiments.

We have already indicated the part of this work which relates to the animal organism, as containing some valuable speculations well and clearly stated. The conclusion at which Liebig arrives in this part of the work is, that, according to the laws of endosmosis and exosmosis, the cutaneous transpiration and respiration must exercise a more considerable influence than has been generally admitted, upon the motion of the fluids in the vessels; that these fluids, indeed, must be drawn towards the skin and the lung with considerable force to supply the loss of fluid in these situations. That the pulmonary and cutaneous exhalations are conformable to the physical laws of endosmosis and exosmosis, and are probably very much promoted by these forces, there can be no doubt. The experiment of Liebig, illustrating this point, is very ingenious; it is, however, nearly the same with one performed by Professor Leslie of Edinburgh, in connexion with a different subject. If a bent tube, widened at one extremity, and closed at this end by a piece of moist bladder, be filled with water, and the narrow end be then placed in a vessel of mercury, evaporation will take place from the surface of the membrane, and the volume of the water within will be diminished. At the same time, air will pass slowly into the tube, and partially replace the water. Nevertheless, it will be found that a pressure from within outwards is generated, sufficient to raise a column of mercury in the smaller end of the tube from 12 to 24 inches, according to the thickness of the membrane. This pressure, of course, is generated according to the law of exosmosis.

Leslie's experiment was nearly identical with that of Liebig, the only difference being, that the evaporating surface was formed by a thin ball of porous earthenware to which a glass tube was adapted. This instrument was filled with water, and inverted in mercury, as in Liebig's experiment; the same results were produced, thus showing what Matteucci's experiments so clearly demonstrate, that exosmosis is a purely physical phenomenon.¹

In the animal body a similar process goes on. There is a perpetual evaporation from the surfaces of the skin and lungs, which tends to diminish the volume of the fluids; and there is likewise an absorption of gases from the atmosphere into the blood. In so far as this process, therefore, follows a physical law, it is perfectly clear that there must be a constant pressure exerted towards the surface.

Liebig has not investigated the relations of the skin in animals which live in water, to the physical processes of endosmosis and

¹ This instrument was used by Leslie for determining the rapidity of exhalation from a porous surface.—See *Encyclopædia Britannica*, article *Atmometer*, and Leslie's *Short Account of Experiments and Instruments depending on the Relations of Air to Heat and Moisture*.

exosmosis. In relation to this branch of the subject, Matteucci discovered the remarkable fact, that, in the skins of most amphibious animals, endosmosis takes place much more actively from the internal to the external surface, than in the contrary direction. The application of this fact to physiology is obvious. Some differences in this respect also exist in nearly all animal membranes, the transudation being found to differ in rapidity, according as the one side or the other is placed in contact with the denser liquid. The ignorance of this fact by Liebig is an unfortunate circumstance, as it may have unconsciously interfered with the results of some of his experiments.

The influence of these physical circumstances upon pathological processes, is certainly worthy of attention. It is clear, that the physical effect of diminished transpiration, must be diminished motion of the fluids towards the surface. This circumstance, in Liebig's opinion, may be explanatory of many diseases, and is an obvious cause for the relation of diseases to the hygrometric state of the atmosphere. Not only is this the case in animals, but in plants. The blight of hops and potatoes may probably be owing, like the influenza in the human subject, to suppressed transpiration. A considerable space is given to the observations of Hales in reference to the hop-blight, and the motion of the fluids in plants.

We refrain from pursuing this subject further at present. These speculations seem to be sufficiently important; but, in the crude theoretical form in which they are here presented, it would be impossible to make them interesting to the majority of the profession. They are undoubtedly capable of being submitted to the test of direct experiment. Indeed, the researches of a recent observer (Fourcault) have been directed to the careful experimental investigation of the effects of suspended transpiration. We shall probably soon be able to lay these results before the readers of this Journal.

Recent Advances in the Physiology of Motion, the Senses, Generation, and Development. By WILLIAM BALY, M.D., F.R.S., Physician to Millbank Prison, and Lecturer on Forensic Medicine at St Bartholomew's Hospital; and WILLIAM SENHOUSE KIRKES, M.D. Being a Supplement to the Second Volume of *Müller's Physiology*. London, 1848.—Pp. 132.

THIS exceedingly careful and elaborate compilation forms a useful supplement to the second volume of Müller's Physiology, a work which, although not, perhaps, the most readable and generally useful in our language for the beginner in physiological science, must long maintain its place in English literature, as the most trustworthy and satisfactory guide for the advanced student. The same spirit of careful and impartial research, which formed the distinguishing characteristic of the original work, appears to have

guided the authors of the volume before us; the details of which are so varied and numerous, as to render it impossible for us to go into them at any length.

We hope that ere long Dr Baly will turn his attention to the subjects contained in the first volume of Müller's work. Some of these, such as Digestion and the Nervous System, require revision no less than those in the second volume. Meanwhile we have much pleasure in recommending the work of Drs Baly and Kirkes to our readers, as being at once well executed, handsome, and moderate in cost.

Part Third.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXVII.

MEETING XIII.—Wednesday, July 5, 1848.—Dr D. MACLAGAN in the Chair.

ON SEVERAL NEW GALVANIC BATTERIES. BY DR WRIGHT.

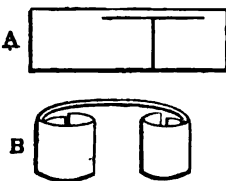
1. In the *Monthly Journal* for July, I inserted a short notice of this new battery, as I wished to publish as soon as possible the principle on which it was founded, viz. *the inactivity of zinc in a mixture of concentrated nitric and sulphuric acids*. I now proceed to give an account of the mode of arranging the apparatus, sufficiently detailed to enable any person to construct it, as the battery is especially adapted for medical purposes, on account of its high intensity, and the facility with which it may be put together.

2. The battery essentially consists of a positive plate of zinc associated with a negative plate of the same metal covered with platinum, the former immersed in a solution of some neutral salt or caustic alkali, the latter in a mixture of nitric and sulphuric acids. The intermixture of the two liquids is prevented by the intervention of a porous cell, which may be formed of garden pot-clay, pipe-clay, or plaster of Paris. The cheapest and most convenient form of the apparatus is constructed in the following manner:—Take a common jelly jar two and a half inches in diameter, and line it with a strip of the thinnest sheet zinc two inches wide, from the edge of which a narrow strip or ear is cut, turned over the edge of the jar, and twisted to a copper wire (No. 20); within the zinc place a small porous earthenware garden-pot rendered water-tight by a plug of sealing wax, and within this again a roll of platinized zinc, to which a similar wire has been twisted. The battery is now complete; on charging it, pour a saturated solution of common salt or muriate of ammonia in the space without the porous cell and within it, and in contact with the platinized zinc the nitro-sulphuric acid.

3. The nitro-sulphuric acid consists of *nitric acid one part and sulphuric acid five parts*. In such a mixture zinc, the one of the most oxidizable metals, is as inactive as a plate of glass or porcelain; but a very slight deviation from the above proportion by the addition of nitric acid or water dissolves the charm, the metal is acted on, and the platinum is thrown off its surface, for which reason it is advisable to let the nitro-sulphuric acid have an elevation within the porous cell, higher than that of the saline solution without it.

4. I have now described the mode of constructing a single cell of the battery, which is well adapted for use in conjunction with the electro-magnetic coil machine; but when the instrument is to be applied to the decomposition of saline or other fluids, it is proper to use from four to six cells in series, the

zinc roll of one cell being in connexion with the platinized zinc of the cell next to it. In constructing a compound battery of this kind, the alternate cylinders of zinc and platinized zinc are cut from the same slip of metal by a T-shaped incision, and bent into rolls as in the figure, in which A indicates the zinc cut, and B the same slip bent into the proper form, the small cylinder being afterwards platinized nearly up to the top: the zinc and platinized cylinders at each extremity of the series are of course unconnected, and have wires twisted to them to serve as poles or conductors. I think it will be at once understood that screws or joinings are unnecessary in this battery, however extended the number of pairs in series may be.



5. The zinc is easily platinized, after having been cut and moulded into the requisite shape, by dipping it for a few seconds in a solution consisting of

Saturated solution of chloride of platinum,	3 ss.
Sulphuric acid,	3 jss
Water,	3 ij

it is then washed and quickly dried, and is found to have assumed a smoky tinge from the minutely divided platinum adhering to its surface.

6. For the administration of the galvanic shock and current, I use a small battery constructed in the following manner:—A plate of thin sheet zinc three inches by four is platinized to half its width, and cut into twelve slips, which are bent into the form of the letter U, the dark leg of which represents the platinized leg of the bent slip. Twelve of the glasses used in pewter ink-stands are then filled with moistened table-salt, and a tobacco-pipe bowl, rendered water-tight by sealing-wax, is thrust into each of them, and instantly filled with the acid mixture (3), before the saline solution has time to percolate through it; the glasses are then ranged in a double line, and the zinc arcs placed in them, each having its platinized leg in the acid mixture, its unplatinized leg in the salt of the glass next in series; fine iron wires twisted round the slips at each extremity are conducted into basins of salt and water, from which the shock may be taken by immersing the hands therein. This small arrangement affords a stronger shock than a Cruikshank battery of fifty pairs of plates, accompanied with a strong sensation of heat in immersing and removing the hands from the basins.

7. Several forms of the platinized zinc battery were brought by me before the Medico-Chirurgical Society of Edinburgh, the apparatus used being a single cell (2), in conjunction with an electro-magnetic coil machine; a compound battery of four cells (4), the poles of which terminated in platinum plates immersed in dilute sulphuric acid, and the small compound battery (6). The sparks from the first of these arrangements were most brilliant, the second decomposed the water of the dilute acid with great rapidity, and the shock from the third was perfectly satisfactory to several of the members who experienced it, even after the battery had been charged more than three hours.¹

8. In an economical point of view, this battery is much superior to any other; its intensity is four times greater than that of Smee's battery, the arrangement now in common use, at about a fiftieth part of the cost, as it can be put together by any one having a little sheet zinc and a pair of scissors. The saturated solution of platinum is about 2s. per oz., a sixteenth part of which quantity is sufficient to platinize more than a dozen cylinders.

9. The cells should always be well washed after use, during which process a great part of the platinum is removed; it will therefore be advisable to dip the cylinders in the platinum solution before again using them.

¹ The electro-magnetic coil machine used on this occasion is constructed on a new principle, and will be described in an early number of this Journal.

Fig. 1.



Fig. 2.



Fig. 2.

- a. Sternum reflected.
- b. Left Fetal Heart.
- c. Right Fetal Heart.
- d. Common Pericardium.
- e. Common liver with suspensory ligament.
- f. The Lung.
- g. Common Diaphragm.
- m. n. Umbilical Veins.

ON CURABLE HEADACHE, AS REGARDS ITS PATHOLOGY AND TREATMENT. By Dr SELLER.—This communication will be inserted entire in a future Number.

SINGULAR CASE OF DILATATION OF THE DUCTUS COMMUNIS CHOLEDOCHUS. By Dr HALLIDAY DOUGLAS.—In this case the ductus communis choledocus was so dilated as to hold half a gallon of fluid; during life it had been punctured. The case itself will be given in a future Number.

ON THE EXHIBITION OF CHLOROFORM IN DENTAL SURGERY. By F. B. IMLACH, Esq.—This paper will appear in a future Number.

EDINBURGH OBSTETRIC SOCIETY.

SESSION VII.

MEETING V. May 10.—Dr SIMPSON, President, in the Chair.

BIRTH OF A DOUBLE MONSTER. BY DR LYELL. COMMUNICATED BY PROFESSOR SIMPSON.

Dr Simpson laid before the Society a case of the birth of a double monster, which occurred in the practice of Dr Lyell of Dundee, who had kindly forwarded the infants to him for examination. The monster belonged to Geoffroy St Hilaire's family of *Monomphaliens*, characterised by the union of two almost perfect individuals having a common umbilicus, and was of the genus *Sternopage*, in the same classification. It agreed with St Hilaire's characters of the genus in the junction or fusion of the two foetuses, extending from immediately below the umbilicus to the apex of the sternum, (Fig. 1); in having a common pericardium (*d*, Fig. 2) containing two distinct and perfect hearts (*b c*, Fig. 2); in having four perfect lungs; but it differed from St Hilaire's generic description in this one particular, that the cavities of the pleuræ were not fused and thrown into one, but remained separated by a double serous layer, composed by the reflexion of the pleuræ. There was a single diaphragm (*k*), and a large liver (*e f*) common to both lying in the mesial line, and over which the two peritoneal coatings were reflected, one over each half, forming also its suspensory ligament in the mesial line above, and the septum between the two abdominal cavities by their union below. From the liver, at both sides of the serous dissepiment of the two peritoneal cavities, issued an umbilical vein—(*m n*). These, with their corresponding umbilical arteries, converged to form the cord which issued single from the umbilicus, and at a length of about eight inches divided into two stems, each of about two inches long, which were inserted separately into a common and single placenta.

The history of the labour Dr Simpson laid before the Society in the following letter from Dr Lyell, received a few days after the woman's delivery:—

"On the evening of Monday the 2d November 1846, I was called about half-past eight P.M., to attend Mrs A—, in labour with her second child. She had her first child, a boy, twenty months ago; and then her labour was so quick that no doctor was sent for. On my arrival about nine o'clock, I ascertained that she had been ill about three quarters of an hour. On examination, I found the vertex presenting, the os uteri dilated to the size of a crown-piece, and very dilatable. She complained of the pains being strong; but as they were not at all expulsive, I ruptured the membranes. As this did not mend matters, I exhibited a dose of ergot, but it had no better effect. A hand was felt coming down along the side of the head. From the circumstances of the pains being so strong, and their being no appearance of the head being impacted, I began to suspect there were twins, and that the one child was, in some way, preventing the delivery of the other. I, therefore, thought it would be necessary to give her some assistance with the forceps. At twelve P.M. I proceeded to apply them, and succeeded in effecting the delivery of the head in a few minutes, but I could not make the body descend, and when I stopt pulling, the head was forcibly drawn back against the external parts. I tried pretty strong trac-

tion again, and the neck yielded as if the head had partially separated from the body. On throwing aside the bed-clothes, and examining the head, I discovered the presence of harelip and cleft palate in the child, and as it made several convulsive movements, I was anxious to effect the delivery as soon as possible, that its life might be saved. I brought down first one arm and then another, but still could not get away the body. The face looked towards the pubis. At this time I felt a third hand descending, and now there was no doubt of there being a second child. I introduced my right hand for the purpose of changing its position, but found it so wedged, lying across the brim of the pelvis, that its position could not be altered. The lower part of the body of the half-delivered child lay across the loins of the second, and was thus prevented entering the brim. I withdrew the right hand, and, after reflecting for a little, introduced the left hand along the spine of the first child to the nates, and succeeded in dragging its body into the pelvis, but all advance was again stopped. Anticipating the necessity of turning the second child, I seized and dragged its limbs along with the still undelivered limbs of the first child, and at last, after many efforts, succeeded in completing the delivery at half-past twelve P.M., the two infants revolving, as it were, upon their common sternum, around the symphysis pubis of the mother. From the first application of the forceps to the completion of delivery, from twenty to twenty-five minutes elapsed. The first child having the harelip was alive, and lived eighteen hours. The second was born dead. They were of full size (eighteen inches long), and, on the whole, well-formed. The mother made a good recovery. Mrs A—— has since been delivered of a healthy child at the full time."

The case, Dr Simpson observed, was interesting *obstetrically* from the accurate description of the mechanism of the delivery as detailed by Dr Lyell, there being few details yet on record of the mechanism of labour with double monstrosity. The practice adopted by Dr Lyell seemed the very best that could possibly have been adopted. The wonder was the frequency with which double monsters were born without almost a mother ever being lost. In most cases, the foetuses seemed to overlap, and adapt themselves to the inequalities of each other; and, when turning or extraction by the feet was adopted, it was usually found that pulling by the feet of one body (after all the four were down) was preferable, as it allowed the one body to overlap the other, and the head of the first child, for instance, to be pulled down, and adapted to the neck of the second, thus greatly saving size and space.

The case was interesting *physiologically*, as it formed, he believed, the first recorded instance of any of the infants in a case of *Sternopage* monstrosity living beyond a few seconds after birth. In general the two hearts are too intimately fused together to allow of the survival of either of the infants after birth.—(To be concluded in our next.)

ELECTION OF DR HUGHES BENNETT TO THE CHAIR OF THE INSTITUTES OF MEDICINE
IN THE UNIVERSITY OF EDINBURGH.

On Thursday, July 13th, the Magistrates and Town-Council of Edinburgh met for the purpose of electing a Professor of the Institutes of Medicine in the University, vacant by the resignation of Dr Allen Thomson.

Dr Gunning and Dr Martin Barry having withdrawn as candidates, the Lord Provost rose and said,—I am happy to have it in my power to propose for this vacancy a gentleman who has shown his qualifications in no ordinary degree, by his writings and by his lectures, upon the branches of science which are to be taught in that chair. It would be a waste of time were I to refer to the many very high testimonials which he has obtained, not only from scientific men in Great Britain, but in all the great schools in the continent of Europe. We may consider ourselves exceedingly fortunate in having had a candidate so well qualified, not only to maintain, but to advance, the reputation of our University, and

I hope that this election will be unanimous. At one time I certainly was wavering in my own mind regarding the respective qualifications of Dr Barry and Dr Bennett, the gentleman I am now to propose. Dr Barry has brought forward testimonials of a very startling kind, and his own gentlemanly bearing, and his high character, were such that one might have been proud to have had such a professor in the University. However, I wish it to be distinctly understood that it was not the idea of the tests, whether they were to be put or not put, that had any weight upon my mind; for had I considered that Dr Barry was best qualified I should have proposed him, whether he would have taken the tests or not. It was my intention that we should not have mooted the question of the tests at all; and, had any gentleman come forward and made inquiry about that question, I should have stated that, at this stage of the proceedings, we had no opportunity of making any such inquiry. I do hope that candidates in future will not be deterred from offering themselves on account of these tests; because, if Dr Barry had been considered the best qualified, it would have been a great credit to himself to have had the majority of the electors on this occasion. I made up my own mind independently of this question; after having examined the various testimonials, and after making inquiry of medical gentlemen in Edinburgh, I was satisfied that it would be impossible to get any one so well qualified for that chair as Dr Bennett. It is unnecessary to take up your time upon this question: he has not only shown, both by his writings and his lectures, his thorough qualification for the situation, but we know that he is so industrious and so persevering, and his ardour and zeal are so great, that we have every reason to believe that he will go on making further discoveries in science, and will be a credit to the University, and will be the means of advancing its fame in the world. I have therefore very great pleasure in proposing, that Dr Bennett be elected to fill the vacant chair of the Institutes of Medicine.

Mr Melville had much pleasure in seconding the motion. He was thoroughly convinced that Dr Bennett would add to the reputation which the University had already acquired. They owed him a debt of gratitude for the benefits he had conferred on the academic school. He had no doubt that the fame he had there acquired would go with him to the University. With regard to the tests, this was not the time to put them. It was their duty to hold, that every person making such application will comply with the requirements which the law prescribes. He trusted that on all future occasions they would assume, that those who came forward were prepared to comply with the law.

Mr Gulland said, there could be no doubt that the mere circumstance of these tests existing had a tendency to prevent gentlemen from coming forward who would otherwise do so; and he therefore thought that they should urge government to bring in a bill for the immediate abolition of these tests.

Mr Ritchie just wished to express his admiration of Dr Bennett. He had attained his high standing in life by his own energy; he is a man who will work his way through life by his own individual talent, as he had placed himself in the sphere he now occupies by his own merit.

Dr Renton thought that justice should be done to all parties. In reference to Dr Barry, he would say that this board is fortunate in having such gentlemen to come forward to fill the chair of physiology. Dr Barry had two certificates from the highest medical authority in Great Britain; and although he did not possess the same amount of energy as Dr Bennett, he comes forward as an original thinker, a sound-headed man, one whose zeal is indefatigable, and he knew of no limitations to such a mind.

Mr Dick was satisfied that either of them would have done high honour to the University, while he considered that Dr Bennett was superior to Dr Barry. He would not have risen but from the fact, that in regard to the tests he was in the minority in the Hebrew chair question; but, had Dr Barry been elected to the chair, he would have considered it his duty to allow him to go into the University without any mention of the tests at all.

Dr Spittal said he had had various occasions of bearing testimony to the know-

ledge of Dr Bennett, and he highly congratulated the council on the admirable choice they had made. In regard to the claims of the two gentlemen alluded to, he not wish to enter into any comparison; but it was evident that Dr Bennett stood so prominently forward, as far to outstrip the claims of Dr Barry. Dr Bennett's progress and success in teaching he had watched with great interest, ever since he commenced his labours in Edinburgh. His claims as a teacher of practical medicine are of a very superior description; and as instruction in clinical medicine will form part of the duties of the future professor, he felt satisfied that Dr Bennett will revive the celebrity of the University in this very important department.

The Provost's motion was then unanimously agreed to.

Mr Clark said, that the appointment now so unanimously made, he was quite sure, could not be more gratifying for Dr Bennett to receive, than it was on the part of the patrons to bestow.

BOOKS RECEIVED.

1. A Treatise on the Practice of Medicine. By George B. Wood. M.D., &c. 2 vols. large 8vo. Philadelphia. 1847.
2. Lectures on Diseases of the Eye. By John Morgan, F.L.S., &c. Second Edition, carefully revised and enlarged with Notes, by John F. France, &c. London. 8vo. 1848.
3. A Practical Treatise on the Diseases Peculiar to Women. By Samuel Ashwell, M.D., &c. Third Edition. London. 8vo. 1848.
4. The Retrospect of Medicine. Edited by W. Braithwaite, &c. Vol. XVII. London. Small 8vo. 1848.
5. Memoranda for Young Practitioners in Midwifery. By Edward Rigby, M.D. London. 24mo. 1848.
6. The Journal of Psychological Medicine and Mental Pathology. Edited by Forbes Winslow, M.D. No. 3. London. 8vo. 1848.
7. The American Journal and Library of Dental Science. April 1848. Baltimore. 8vo. 1848.
8. The Ethnological Journal. July 1848. Edited by Luke Burke, Esq. London. 8vo.
9. Eighteenth Annual Report of the Belfast District Asylum for the Insane Poor. 8vo. Belfast: 1848.
10. Hassall's Microscopic Anatomy. Part XIII.
11. Littel's Living Age. No. 201. 18th March 1848. Boston.
12. Harvey and Buchanan's new and improved Synoptical Table of the Diseases of the Human Ear, &c. 1848.
13. Zeitschrift für die Gesamnte Medizin. Von Oppenheim. Band 37. Heft. 3.
14. Bibliothek for Læger, Tredie Række. af. H. Silmen. Kjobenhavn. Oktober 1847.
15. Report of the Pennsylvania Hospital for the Insane, for the Year 1847. By Thomas S. Kirkbride, M.D. Philadelphia. 1848.
16. The Nineteenth Annual Report of the Inspectors of the Eastern State Penitentiary of Pennsylvania. Philadelphia. 8vo. 1848.
17. Twenty-seventh Annual Report of the Bloomingdale Asylum for the Insane. By Pliny Earle, M.D. New York. 8vo. 1848.
18. History, Description, and Statistics of the Bloomingdale Asylum for the Insane. New York. 8vo. 1848.
19. American Journal of Dental Science. January 1848.
20. Outlines of Lectures on the Nature, Causes, and Treatment, of Insanity. By Sir Alexander Morrison, M.D., &c. Fourth Edition, with Plates. London. 8vo. 1848.
21. General Index to the British and Foreign Medical Review. Edited by John Forbes, M.D., &c. Vol. XXV. London. 8vo. 1848.
22. Practical Observations on the Administration and Effects of Chloroform, especially in its Application in Cases of Natural Labour. By J. H. Stattard, Esq. &c. London. 8vo. 1848.
23. A Plain Statement with Reference to Medical Reform. By Edwin Lee. London. 8vo. 1848. *This Pamphlet is distributed gratuitously to the profession.*
24. A Report on the recent Progress of Psychological Medicine, and Mental Pathology. By C. Lockhart Robertson, M.D., &c. Small 8vo. 1848.

We have to express our acknowledgments for several copies of the China Mail, and of other Newspapers.

ERRATA IN JUNE AND JULY NUMBERS.

In Dr Basham's evidence, June Number, p. 901, 19 lines from top, and

In the July Number, p. 62, 20 lines from bottom of page, for "absorption," read "deposition."

NOTICES TO CORRESPONDENTS.

Articles by Mr JOSEPH BELL and Dr KELBURNE KING are in type, and will appear in the next Number.

THE
MONTHLY JOURNAL
OF
MEDICAL SCIENCE.

No. XCIII.

SEPTEMBER, 1848.

No. 27. NEW SERIES.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Seat of Headache in the Sympathetic Nerve, and on some of the Rules of Treatment, drawn from its connexion with Chronic Ill Health.* By WILLIAM SELLER, M.D., F.R.C.P.E., Extraordinary Physician to the Royal Infirmary of Edinburgh, &c.

WHEN headache is dependent on disease of the membranes of the brain, the cranial bones, or the scalp, the pain plainly follows the usual law of pain. The cause of the pain has its seat in one or other of these several textures, and the consciousness of the sensation is referred to that seat. But when the cause of pain has its seat any where in the encephalon itself, it becomes a question what the organs are through which the consciousness of the sensation arises. Of the several parts composing the encephalon, some, and these form the largest portion, may be cut or punctured in living animals without any discoverable effects; others cannot be so treated without the occurrence of paralysis or convulsions; others again, when dealt with in the same manner, are the source of pain; but the consciousness of that pain is not referred to the parts acted on, but to the extremities of nerves originating in those parts. We should expect, then, that the effects of disease within the encephalon in man, would correspond to these results of experiments on animals. That acute inflammation, for example, should affect the greatest part of the encephalon, without giving rise to pain at all—that when the same invades a certain number of parts, paralysis or convulsion should occur without pain; and that when it seizes any of the remaining parts, pain should uniformly arise, referred, however, not to any part of the encephalon, but to parts supplied with sentient nerves from the encephalon.

NEW SERIES.—NO. XXVII. SEPT. 1848.

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No such rule, however, is observed. And if the facts recorded in the pathology of the brain be attentively considered, it will be found, I think, that headache has been remarked along with uncomplicated disease of almost every particular part of the encephalon.¹ Yet it remains a question whether the consciousness of pain be referred in these cases to the affected part. This question cannot be determined in a direct manner, because such a reference cannot be judged of with exactness in the encephalon any more than in the other organs, of the changes on which we have not been rendered conscious in the state of health. The advantage of direct observation is, therefore, unavailable in this instance.

There are, I think, only three conceivable modes of explaining the occurrence of headache in affections of the encephalon itself, consistently with the established laws of the animal economy. In one of these the assumption is made, that the reference of the consciousness of pain is not to the seat of the actual irritation, but to the extremities of the nervous filaments in the adjacent membranes or the scalp; in the two others, the reference is held to be made to the real seat of the cause of pain.

The first is at present the common view, though it does not appear to have met with a very searching investigation from physiologists. It is dependent on what is termed the reflection or radiation of sensation.² A typical example of this reflection of sensation occurs in toothache. The acute impression made on a few filaments of the fifth pair of nerves, exposed by caries of a tooth, being transmitted to the origin of these filaments in the encephalon, excites not merely the usual change necessary for the consciousness of pain, and its reference to their extremities in the carious portion of the tooth, but flows over on the adjacent parts of the encephalon, where other filaments of the same nerve take their rise. These being thus stimulated in the same manner as those of the affected tooth, give origin to the like sensation; and the consciousness of this accessory sensation of pain is referred to the whole cheek of the same side; and in this way is explained the diffusion of the pain. In like manner, in symptomatic headache, for example, the extremities of the par vagum in the stomach, or of the splanchnic or sympathetic nerve in the bowels, liver, or uterus, being irritated, the impression is conveyed to the origin of the affected filaments in the nervous centre; and being supposed to spread to the other parts of that centre, where sentient filaments, distributed to the dura mater or to the scalp, originate, gives rise to the consciousness of pain there. And similarly, according to this view, when the cause of pain is seated in the encephalon itself, the irri-

¹ See Abercrombie on Diseases of the Brain, cases 53, 54, 55, 56, 57, 58, 60, 69, 100, 106, &c.

² Todd and Bowman, Physiology, vol. ii. p. 368; also, Part Third, p. 115. Müller, Physiology, by Baly, vol. i. p. 697.

tation is supposed to spread to the region where nervous filaments of the dura mater, or of the scalp, originate; and thence the consciousness of pain is referred to those parts. The objections to this mode of explanation will be stated after a short consideration of the remaining views.

The second view assumes, that though the encephalon be generally destitute of sensibility in health, yet, like some other parts, it may become sensible under certain morbid conditions, and that this sensibility is independent of nerves, being altogether *sui generis*. Thus the hemispheres of the brain are in so far concerned in every sensation of pain, from whatever source, that though they may not be necessary, as some think,¹ to the mere consciousness of the sensation, yet they are necessary to the remembrance of the consciousness. But as in man, when awake and free from the influence of such agents as chloroform and ether, there is uniformly the remembrance of pain, the hemispheres of the brain must be admitted to be ordinary agents in every complete sensation of pain. When, then, a sufficiently powerful morbid cause operates on the substance of the hemispheres, there may be, according to this view, what is equivalent to a concentration into one of all the several ordinary steps or stages in the process of sensation; these are, 1, an impression on the extremity of a nerve; 2, the extension of that impression to the sensory tract of the nervous centre; 3, a primary physical change there; 4, a transmission upwards through the white filaments to the hemispheres; 5, a secondary physical change there; 6, a consciousness complete enough to be remembered. Or the second view simply amounts to this, that the hemispheres, being the essential seat of sensation, must be capable of sensation, at least when the impression is of sufficient force, even though the subordinate instruments in the ordinary process cannot come into operation. Of this view also hereafter.

The third conceivable view of this question is that here proposed for adoption. It is contained in the three following propositions:—1, That the susceptibility to pain, under certain morbid conditions—some temporary, some permanent—is no special endowment of the substance of the encephalon, but the result of the ordinary law of susceptibility to pain in the vascular solids. 2, That it is not the encephalon, either in its grey or white matter, or the nerves concerned in its proper functions, which are subject to the first effect of irritation in the production of headache, but the nerves subservient to its vitality as an organ of the animal machine. 3, That these nerves are the filaments of the sympathetic, diffused throughout the encephalon in company with the nutrient capillaries.

How far these views bear examination must now be considered.

¹ Grainger on the Structure and Functions of the Spinal Cord, p. 11.

To go back to the first—the case of headache which most distinctly suggests the reference of the sensation to the cranial nerves, is the pain over the left brow in derangement of digestion. This pain is instanced by Todd¹ as a well-marked example of reflected sensation, the primary impression being conveyed to the nervous centre through the par vagum, and thence reflected to the extremities of the frontal branches of the fifth pair. In this explanation it does not appear why the highest branches, namely, the frontal branches of the ophthalmic portion, should be the channel of the reflected sensation, rather than the branches of the second portion and the sentient branches of the third portion, the points at which these originate being obviously nearer the part of the nervous centre disturbed by the impression conveyed from the stomach through the par vagum. Nor does it appear that there is any greater direct evidence in favour of this explanation which makes the case an anomaly, or at least exceptional, than for the supposition of the pain being really in the anterior part of the left hemisphere, consequent on irritation of the gastric filaments of the left vagus nerve, which keeps it within the general law. It is impossible to deny, on just grounds, that phenomena entitled to be termed *reflected sensations* do occur; but it is an error to regard these as the result of a law extensively applicable to the acts of the animal economy. They seem to arise under the unusual stimulation of sentient nerves, and differ widely in character from *reflected motions*. Without the constant operation of the great law on which reflected motions are dependent, the animal machine would quickly come to a stand. On the contrary, the phenomena falling under reflected or radiated sensations, are, as it were, erroneous acts of the system, comparatively rare, incidental, forming no part of the ordinary operations in its economy, the result almost uniformly of morbid causes. The conditions on which the unequivocal examples of reflected sensations arise, are obviously the intenseness of the impression made on the extremities of the nerve first affected, and the proximity in the nervous centre of the origin of the nervous filaments, through which the reflected sensation is conceived to be conveyed. But these conditions are not obviously present when irritation of the stomach is supposed to be transferred through the par vagum to the first portion of the fifth pair of nerves, and still less when irritation of the bowels, liver, or uterus is represented as exciting headache on the like principle. If there be affection of the encephalon at the origin of the fifth pair of nerves, then there may be pain of the scalp, independently of any radiation of sensation, or in accordance with the general law.

The second view need not detain us. It must be regarded merely as a last resort, in case the two other views should be dis-

¹ Todd and Bowman, Physiology, Part Third, p. 115.

proved. It violates the ordinary simplicity of physiological laws. Yet were it discovered that there is no such phenomenon as the reflection of sensation, and that no nervous filaments accompany the encephalic capillaries, while it distinctly appeared that the chief part of the substance of the encephalon becomes, under certain conditions, the immediate seat of the consciousness of pain, then we should be compelled to admit two laws of sensation, one in which nerves are essential instruments, and one direct, without the intervention of nerves. But no such necessity is likely to arise.

As regards the third view, which ascribes headache affecting the encephalon to the ordinary process of sensation, exercised by means of minute nervous filaments spreading from the sympathetic throughout its substance, it must be admitted that the existence of such nerves cannot be demonstrated. Nevertheless, the evidence falls little short of demonstrative. It is not doubted by our best authorities, that the sympathetic is distributed in the rest of the body co-extensively with the vascular system. Our most recent accounts of the process of inflammation, to which all vascular parts are subject, assign an important share in that process to the nerves of the capillaries. What would become of these views did the capillaries of the encephalon possess no nervous filaments? If the conclusion be all but inevitable, that the sympathetic is spread elsewhere co-extensively with the bloodvessels, nothing but a confusion of ideas as to the endowments of the nervous substance could lead to the belief of an exception in regard to the bloodvessels of the encephalon. But the force of this evidence, as far as it goes, being admitted, it may yet be said that the nervous filaments accompanying the capillary bloodvessels are not sentient but organic nerves—nerves not destined to become susceptible of pain, but for the maintenance of nutrition and a high vitality. The sympathetic has all the characters of a nerve adapted for presiding over organic rather than over animal acts. Yet it contains both motor and sensitive filaments. It is an incontrovertible fact, that parts supplied exclusively with nerves from this source—for example, the bowels—are susceptible of the most acute pain.¹ It is not unlikely that the general feeling of bodily wellbeing, and that of bodily discomfort, are dependent on opposite states of the vascular filaments of this system of nerves; and, indeed, Bouillaud has expressly ascribed the latter feeling to such a cause. It is not necessary for the object before us to settle whether the sympathetic be an independent portion of the nervous system, and what is the nature of its connexion with the cerebro-spinal axis. It is enough if it shall appear that there are sentient filaments in the sympathetic; and that

¹ See Swan on Diseases of the Nerves, 1834, p. 289; Grainger on the Spinal Cord, p. 136; Müller, Physiology, by Baly, vol. i. p. 742; and Todd and Bowman, Physiology, Part Third, p. 143.

these filaments, distributed along with the encephalic capillaries, are the immediate instruments of sensation in headache affecting the substance of the encephalon, whether the primary central acts be dependent on the cervical ganglia, or on the cervical portion of the spinal cord. The small amount of nervous substance contained in the plexuses surrounding the carotid and vertebral arteries, hardly forms any objection to this view, when it is considered that, compared to its volume, the encephalon, owing to its unusual supply of blood, and the general fluidity of its constituents, contains but a small proportion of matter in a solid state of organization, or in a form capable of having sensibility imparted to it.

It is a valid reason in favour of this view, that the same principle of explanation is applicable to what seems at first sight an anomaly in the observations made on the spinal marrow, and the trunks of the nerves of sense. The following is Muller's statement of this case—"An apparent contradiction occurs in the nerves of sense and in the spinal cord, namely, that there is sometimes pain in the part to which the irritation is applied, as well as in the extreme filaments, and that pain is not merely felt in all the parts which receive nerves from below the seat of a lesion of the spinal cord, but that the injured or diseased part itself is painful. * * * We are at present ignorant why sensations should at one time be felt in the peripheral parts; at another, in the spinal cord itself."¹ The solution of this difficulty is easy, if it be admitted that filaments of the sympathetic accompany the capillaries of the spinal cord and of the sentient nerves. When the sensory columns of the spinal cord, or the trunk of a sentient nerve, is subjected to any common stimulus, the consciousness of the consequent sensation is referred, in accordance with the ordinary law, to the extremities of the nervous filaments which have thus been stimulated, whether at their origin or in any part of their course. But when a more permanent stimulus operates at the same points, by which the capillary circulation at those points is much excited, then the sensibility of the filaments of the sympathetic distributed there is developed, and the consciousness of the sensation is referred to the extremities of these in the part acted on. Nor is it surprising that a considerable turgescence of capillary vessels should rather affect the nervous filaments spread on their own coats, than the filaments of the nerve which they nourish.

There is, moreover, an objection to the explanation of symptomatic headache by the reflection of sensation, which does not apply to the view here maintained. If a sensation be reflected in the nervous centre to other nervous filaments than those by which the impression was conveyed thither, we should expect that the primary sensation and the reflected sensation would be uniformly of the same kind. Hence, if intense pain of the head be produced by reflection to the nerves of the dura mater, or of the scalp, from a primary irrita-

¹ Muller's Physiology, by Baly, vol. i. p. 802.

tion of the nerves of the stomach, the liver, the bowels, or the uterus, the same kind of sensation—namely, intense pain—should at the same moment exist in one or other of these organs. This, however, is far from being uniformly the case. On the contrary, there is no difficulty in admitting that agents which do not immediately excite pain, may so stimulate the nervous filaments of one or other of those organs, as to disturb the capillary circulation at their origin sufficiently to produce headache in a direct manner.

A few points remain to be noticed which seem to confirm the view which I have been urging. The great cause of temporary headache is obviously local turgescence of the encephalic capillaries. And though the encephalon, owing to its approach to fluidity, be, as a whole, nearly as little compressible as water; yet, in consequence of that almost fluid character, local turgescence of capillaries is more readily admissible than in other organs, the capillaries around the affected part being proportionably compressed. Again, the encephalon being not one organ but a congeries of organs, each endowed with separate functions, upon the ordinary law of excitement under living acts, is liable, beyond other parts, to local determinations. And the encephalon, being the seat of impressions derived from every vascular point of the rest of the living frame, is subject to determinations and other transitory changes, not only from causes affecting directly its own vessels, but from any morbid cause that strongly operates on the nerves even of the most distant parts. Lastly, though causes operating directly within the head, as the effects of intellectual exertions, or of emotions of mind, do very often give origin to such determinations of blood, yet these must more generally result from irritation of the distant extremities of nerves; or, in short, transitory headache is more frequently of what is termed the symptomatic character.

The frequency of speedy relief to headache, when not dependent on serious disease within the head, by the sanative efforts of nature, with or without the aid of remedial measures, has led perhaps to an over-estimate of merely palliative means and temporary treatment, somewhat to the neglect of the principles of a radical cure. A paper of this kind does not afford scope for the discussion of the merits of the ordinary well-known remedies for headache, which I say are chiefly palliative.

The radical treatment, as concerns symptomatic headache, lies in such means as remove sources of irritation throughout the body, and such as so improve the general health as to obviate the renewal of the same irritations. We find headaches occurring under the most opposite states of the system, in the plethoric state, in the anemic, the chlorotic, the scorbutic, the petechial state, the nervous state, the state of increased, the state of diminished, the state of perverted secretion; and lastly, in states of vitiation of the great excretions of the system. Any one of these states, or at least a strong

tendency to any one of these states, may exist without the actual presence of a distinct disease, thus constituting what is conveniently termed chronic ill health. With various diseases chronic ill health combines so as to render the ordinary means of cure unavailing. Symptomatic headache, however, is not so much a disease united with chronic ill health, as a usual part of certain kinds of chronic ill health.

Chronic ill health is a more or less perverted state of the vegetative or assimilative functions, usually consequent on errors of diet and regimen, or on privations and subjection to unfavourable influences arising out of forced occupations. Mere common excesses are not the only errors of diet and regimen, it often happening in modern civilized life, that the habits adopted expressly for the preservation of health become the means by which ill health is perpetuated. It may be observed, that a valetudinarian cast of the constitution being suspected, often on slight grounds, and numerous peculiarities being imagined to exist, the sufferer immediately begins to adopt habits deemed corrective of these, in which he is very prone to deviate far from the standard of healthful regimen. Rules for the supposed preservation of health are seldom safe, when leading to usages much different from the practice of those who keep their health without rules. Between excess and deficiency in the amount of aliment there is but a small interval; and there can be no doubt, that while excess was the prevailing fault in former times, and still is with too many, there are not a few instances met with now in which an under quantity of aliment is taken to the prejudice of the health. The habitual dyspeptic is very apt to sacrifice his general health to the fear of disquieting his stomach, forgetting that dyspepsia can never be cured so long as the stomach participates in a general defect of nutrition throughout the system. Nor must it be forgotten how often a person may indulge in luxurious living without thereby obtaining adequate nourishment.

The fitness of the blood and of the several organs of the vegetative functions to perform efficiently their respective offices, depends on the completeness of the process of nutrition in each. But that process cannot be otherwise than imperfect in chronic ill health. In a chronic state of ill health, then, whether headache or any other source of suffering be the immediate object of attention, the first consideration is, by what means a commencement of improvement may be made, so that, by the gradual application of those, the sanative powers of the constitution may be brought at last to operate without embarrassment. For it is a mistake to suppose, that the physician has any greater direct power over the cure of disease than the surgeon has over the healing of wounds.

The leading principle for the treatment of chronic ill health, and for the permanent removal of the uneasy sensations attendant on it, is the exact adjustment of the food, drink, air, and warmth, to the present necessities of the system.

Chronic ill health may be connected with the plethoric state, in

which event the means of correction are best understood; though even here, perhaps, a nicer adjustment than what the ordinary antiphlogistic treatment affords might be made, by a careful study of each instance in accordance with the principle just laid down. In this case, as well as in the less sanguine and the anemic states of chronic ill health, there may be irritating matters to be evacuated, neutralized, or sheathed, before any progress can be made; for which purposes purgatives, even emetics, antacids, diluents, and demulcents, may be requisite. It is no doubt true, as these irritating matters are continually reproduced while this state of health continues, that evacuants in former times have often been too freely, and even detrimentally, employed, and that diluents and demulcents are of safer use. But it is only the frequent repetition of purgatives that can be hurtful; and the example of trainers for the ring may be regarded as conclusive in favour of the utility of such evacuants as a first step, when the health is to be improved. And though a feeling begins to rise in the profession against strong purgatives, owing to the abuse of these remedies in the earlier part of the century, it is undeniable that some of those, when used with moderate frequency, have the power at once of evacuating acrid matters, and of promoting a more healthy action and secretion in the abdominal organs. Of this description are croton oil, oil of turpentine, and colchicum. The liquor potassæ is the best of the antacids. To their diluent effect is probably owing, in a principal measure, the reputation, in health impaired by dissipation, of some of the less active thermal waters,—witness the Bath water, so much extolled by Heberden in such cases. And it is not impossible that the far-famed repute of the decoction of sarsaparilla for the correction of ill health, which I firmly believe to be well founded, may be owing to no higher quality than its demulcent effect.

I just now referred to the practice of trainers for the ring; and I would now instance their success in improving the respiration, and also the strength and resistance of the muscular system, and, in particular, the action of the skin, as an encouragement to attempt, hardly on similar, yet on parallel principles, the restoration of vigour to the vegetative functions, when impaired by the progress of chronic ill health. The case which comes under the management of the modern trainer, is commonly that of a naturally vigorous young man, whose wind has become impaired, his belly protuberant, his muscles flabby, by all kinds of excesses. A few doses of purgative medicine suffice to relieve the digestive organs from any immediate impediment. After being kept for some time on a regulated diet of plain food, with alternate exercise and rest, he can draw a fuller inspiration, hold his breath longer, and recover it sooner when lost; the muscles become firmer, and contract with force; he is less easily stunned by a blow, and the skin becomes clear, smooth, well coloured, and elastic.

It is, however, only the example of the trainer, not his rules, that we are to follow, in chronic ill health. Every case will require a special direction. It has often been a subject of surprise to medical men, that mineral waters should be of so much efficacy in the restoration of health; but the wonder ceases, when it is considered that, independently of any thing more in the water than a mild diluent quality, the residence at a watering-place, under proper regulation, is nothing else than that kind of training here recommended. A watering-place may be dispensed with, if we can command the same kind of management elsewhere.

For particular morbid conditions, the common well-known rules of treatment are available, too much interference being avoided. In every case where there is the least obscurity as to the nature of the headache, the head should be kept shaved, and cold applied as often as seems to be necessary, to avert unsafe determination of blood. Alterative remedies, as causing least disturbance, are generally best adapted for the ordinary treatment. Under the head of alteratives falls cod-liver oil, which has an inexplicable effect in the promotion of nutrition.

When the excretions assume an anormal character, various well-known remedies are at least of temporary benefit. But neither the bile, the alvine evacuation, nor the urine, can deviate far from the healthy state, without a previous fault in the quantity or quality of the food or drink, in the supply of fresh air, or in the primary processes concerned in the preparation of the aliment; and therefore the first and principal attention, even in these cases, is still due to the means of promoting a healthy nutrition.

ARTICLE II.—*Remarks on Certain Displacements of the Unimpregnated Uterus, with Cases.* By JOSEPH BELL, Member of the Faculty of Physicians and Surgeons of Glasgow, Lecturer on Botany, Anderson's University, Glasgow.

(Read at a Meeting of the Glasgow Medical Society, Nov. 16, 1847.)

THE unimpregnated uterus is subject to various displacements. Descent, prolapsus, and procidentia, are the most common forms; next in the order of frequency we have retroversion and anteversion. Less often we find the organ bent upon itself, constituting either antelexion or retroflexion.

In the following remarks I intend briefly to allude to the causes, the prominent symptoms, and treatment of the four latter forms of displacement:—

1. RETROVERSION.—This condition I have reason to believe very frequently exists. The organ is quite altered in its position—the fundus lies in the hollow of the sacrum—and the os uteri is turned upwards and forwards, either resting upon or above the symphysis pubis.

Causes.—Parturition may be enumerated among the causes of retroversion of the unimpregnated uterus. I have known the affection very frequently follow child-birth; indeed it is by no means difficult to conceive how this malposition will be likely to occur when the female resumes her avocations within a few days after her child is born, a practice very common among the lower and working classes of society. The uterus remains enlarged and the vagina relaxed for several weeks after delivery; the fundus being the heaviest portion, it must necessarily fall in the direction where there is least resistance, viz. the hollow of the sacrum; a distended bladder tending not only to cause but to aggravate the displacement. During seven years of my professional life, I had an extensive midwifery practice among the wives of the working people, and I have found these females very subject to this displacement. It could almost universally be traced to their having left their beds soon after delivery, and to the resumption of their domestic duties.

It is needless to remark, that parturition cannot be the cause of this malposition in those who have never been pregnant. In instances occurring in the unmarried, I have found the uterus either enlarged or its walls occupied by tumours, or the patient had laboured under dysmenorrhœa for years.

I am inclined to consider this affection as a cause not only of retroversion, but also of the other varieties of displacement. Dysmenorrhœa, when protracted, always produces congestion and enlargement of the uterus, conditions tending to change the normal position of the organ.

I have met with a few cases in which retroversion followed an acute attack of metritis; the inflammation had probably subsided into a chronic form, and had gradually produced enlargement, and consequent displacement.

Some may be inclined to view the enlargement of the uterus which almost invariably exists in retroversion, as the result of the altered position, and not the cause. I have no doubt but that the abnormal change does cause congestion and enlargement; judging, however, from my own observations, I am convinced that the organ is generally enlarged more or less before it becomes displaced. These different opinions, however, have no great practical bearing, as we will have occasion hereafter to observe.

Symptoms.—The symptoms may be enumerated in a few words. Difficulty of evacuating the contents of bowels and bladder, particularly the former, pains and a feeling of weakness in loins, often referred to lowest part of sacrum, a sensation of weight about perineum, dragging sensations about iliac and epigastric regions, pains shooting down thighs. These symptoms are aggravated on exertion, on standing, walking, or riding; the patient is often unable to take the slightest exercise without experiencing much suffering; a vaginal discharge often exists, generally thick, not abundant, sometimes coming away in cheesy or curdy masses.

General derangement of the health soon takes place, the patient becomes hysterical; there is irregularity of the menstrual discharge, both as to period and quantity.

On vaginal examination the finger comes in contact with the fundus uteri lying in the hollow of sacrum, the cervix can be traced forward to the symphysis pubis, against or above which the os uteri is detected.

Diagnosis.—The only affections with which retroversion can be confounded are (a) retroflexion, and (b) tumours in the recto-vaginal septum, and in the posterior wall of uterus.

(a).—The diagnosis between retroversion and retroflexion will be pointed out when we describe the latter displacement.

(b).—Besides the form of the tumour, and its connexion with the cervix and os uteri anteriorly, the uterine sound will easily enable any careful practitioner to distinguish retroversion from pelvic tumours (whether ovarian or not), however closely they may be applied to the uterus. If it be the fundus uteri which occupies the hollow of the sacrum, the sound will be found striking against the finger either through rectum or vagina. When using the sound in such cases, I usually place the index finger of left hand on the tumour through the rectum, and thus the other hand has more freedom to direct and use the instrument than when the finger is introduced into vagina. Besides this, in investigating displacements of the uterus, much valuable information will be obtained from the examination being made through rectum as well as vagina. You can in this way examine the posterior surface of uterus, and obtain an accurate notion of the extent of displacement, the size of the organ, its relation to the neighbouring parts, and whether or not it has become abnormally adherent.

As the treatment of the uterine displacements which are to be considered in this paper differs very little, I shall defer my remarks on it until the other forms have been described.

2. ANTEVERSION.—In this misplacement the fundus uteri lies forward either on the symphysis pubis or under it, the os and cervix being turned backwards and upwards towards the promontory of the sacrum. Lisfranc states, that anteversion is infinitely more frequent than retroversion. Boivin and Dugés affirm that it is one of the most common affections to which the uterus is subject.

As regards my own experience, I have met with very few cases. Many instances of considerable obliquity of the uterus have come under my observation. The uterus in its normal situation has its fundus placed towards the pubis, and the neck and mouth are turned backwards, the organ thus having the direction of the superior pelvic axis. Now, this position very often presents a considerable amount of obliquity; but this does not constitute anteversion, which appears to me to consist in that change in the direction of the uterus, when the os uteri is not only directed backwards but upwards towards the

promontory of sacrum, and the fundus turned downwards towards the anterior wall of vagina.

Causes.—I am convinced that anteversion sometimes arises from want of proper rest and care after parturition. Dysmenorrhœa I have known to precede and accompany the displacement.

The only instance which I have had under my care in the virgin state, irregular and painful menstruation had always existed.

Levret, Desormeaux, Lachapelle, and others, hold the opinion, that chronic metritis generally, if not always, precedes anteversion.

Symptoms.—These do not differ much from those of retroversion, some are more prominent, and others less so; in general, dysuria and retention of urine are more severe and frequent than in retroversion.

Diagnosis.—This is so easily formed by a vaginal examination, and by the use of the sound, that it would be a loss of time to enter into details. There is sometimes considerable difficulty experienced on the introduction of this instrument, in consequence of the os uteri being turned upwards and backwards. By introducing a finger into rectum, I have been able to bring down the os uteri, so as to place the sound in the uterine cavity with facility.

Some French practitioners recommended a windowed spoon (*cuiller fenêtrée*) for this purpose.

After this brief sketch of the causes, symptoms, and diagnoses of the “versions” of the uterus, I shall, in the next place, consider the “flexions” to which the organ is subject.

1. *RETROFLEXION.*—Though many writers seem to doubt the existence of this malposition, yet I am convinced that it often occurs. The reason why the writers alluded to have not met with the affection, must be ascribed to the imperfect nature of their investigations: Independent of my own observations, the numerous cases recorded by many acute and careful observers, appear to me to settle the question of the existence of the displacement beyond all possible contradiction. As to its relative or positive frequency, that is quite a different subject, and cannot probably at the present day be satisfactorily discussed. We require additional observations.

The best description of the displacement is given by Denman—it is, “such an alteration in the position of the parts of the uterus, that the fundus is turned downwards between the rectum and vagina, whilst the os uteri remains in its natural situation. An alteration which can only be produced by the curvature of the uterus in the middle, and in one particular state, that is, before it is properly contracted, and when a woman is delivered.”

Causes.—All the cases which I have seen, except two, could be distinctly connected with improper freedom exercised shortly after delivery. In several instances the patient’s sufferings did not fully develop themselves immediately, often a very considerable period elapsed before much inconvenience was experienced. Denman considered that the disease only originates immediately after parturition. Dr Ash-

well is of a different opinion, and has recorded two interesting cases in support of his views. Boivin and Dugés have recorded two cases occurring in young females. Amenorrhœa was the principal cause to which these authors ascribed the affection. It is singular that both of these patients died from hypertrophy of the heart. In one, the cervix uteri was obstructed by chalky matter, and, in the other, the same deposit was found in the Fallopian tubes.

I have met with one case in the virgin state, and another in which an attack of metritis preceded the displacement; but, as I have already observed, these are the only two instances unconnected with parturition that have come under my notice.

Symptoms.—The symptoms differ very little from those of retroversion. The evacuation of the contents of the bowels is attended with equal difficulty and uneasiness in retroflexion as in retroversion. The difficulty of voiding urine is not so much experienced in the former as in the latter. I have met with two instances, however, in which this was the most prominent symptom, being indeed the principal complaint for which medical aid was requested. In these cases the uterus was much enlarged, the os being greatly augmented in size, fissured, ulcerated, and pressing against the neck of the bladder. The sensations of dragging about epigastrium and sides, pain, &c. &c., are also experienced in retroflexion.

When a vaginal examination is made, the finger comes in contact with the os uteri low down in vagina, being nearly in its natural position (generally inclined a little forwards), it is often fissured and ulcerated or abraded; behind it, and lying upon the rectum, the fundus is found in the form of a large tumour. By running the finger backwards from the cervix, the acute angle at which the flexion occurs, is easily detected. The finger through the rectum can be readily passed over fundus, by depressing which the os uteri is tilted upwards and forwards.

Diagnosis.—There cannot be much difficulty in distinguishing retroflexion from retroversion. The position of the os uteri is of itself almost sufficient. In the former it is nearly natural, in the latter it is carried forward and upwards.

From tumours situated in the recto-vaginal septum (of whatever character they may be), also from those in the uterine wall itself, the diagnosis is easily made by the use of the uterine sound, the apex of which can be distinctly felt striking against the fundus of the uterus in its abnormal situation. Sometimes both retroversion and retroflexion are complicated with tumours in the posterior wall of uterus; but the use of the sound, combined with the uneven surface of these tumours, will enable us to decide when such a complication exists.

2. ANTEFLEXION.—This misplacement stands in the same relation to anteversion as retroflexion does to retroversion, the uterus being bent upon itself to such an extent, that its posterior wall becomes anterior, and lies behind the symphysis pubis, the cervix and

as remaining in their normal position. Dr Ashwell states that the point of flexion is usually a little above the union of the cervix with the body of the uterus, and that the degree of curvature determines the slightness or severity of the affection. In the two instances which have fallen under my observation, the flexure took place at the point mentioned by this excellent writer. Antelexion is the most rare of the malpositions to which the uterus is liable. Very few cases have been recorded.

Causes.—In one of the cases which I had under my care, the apparent cause was a very speedy delivery, and perhaps irregular uterine contractions afterwards. The patient was standing on the floor when seized with a pain, and, before she reached the bed, the child and placenta were expelled. She suffered intense after-pains, and shortly after delivery she began to move about the house, when dysuria and other symptoms commenced; this led to a vaginal examination, which detected the malposition. In the other case, the patient had a miscarriage; this was followed by an attack of inflammation of the uterus, and ultimately misplacement.

Some authors enumerate among the causes of antelexion, irregular contractions of the uterine walls after labour, and chronic inflammation. Others ascribe it to direct causes, such as falls, straining at stool, or in the evacuation of urine. I am inclined to think that the latter class of causes would not be likely to produce the affection, unless some abnormal condition previously existed.

Boivin and Dugés have referred it, when occurring in young unmarried females, to rapid and disproportionate growth of the uterus at the period of puberty. This seems to me to be a very powerful cause of the unnatural position; if the posterior wall grow more rapidly than the anterior one, either antelexion or separation of the two walls must occur, and, as the latter is impossible, we must necessarily have the former state produced. This also seems to me to explain how inflammation of the uterus, when that action is confined either to one wall, or a portion of it, produces flexion of the organ. The satyr in the fable expresses his surprise that the man should blow heat and cold alternately with the same breath; it seems equally strange to us, how inflammation should produce two such opposite conditions as hypertrophy and atrophy in organs subjected to its influence. Singular as this may appear, yet such occurrences are most frequent, and may be ascribed to differences existing as to the seat and nature of the deposit effused, and also to the effects produced by such effusion on the processes of nutrition in the organ affected.

With reference to our present subject, it matters not whether the inflammation has produced either hypertrophy or diminution of the whole or part of one of the uterine walls, flexion of the organ must take place. For example, if the posterior wall be enlarged, the fundus uteri will be turned anteriorly; the anterior wall forming the concave, and the posterior the convex side of the curve. The same result must take place if the anterior wall become atrophied. On

the other hand, when the anterior wall becomes enlarged, or the posterior contracted, retroflexion will be produced.

Thus all cases of partial, or of what might be called parietal inflammation of the uterus, if not cured, must necessarily be followed by flexion of the organ; the direction and severity of the flexion being determined by the seat and extent of the hypertrophy or atrophy.

Symptoms.—These differ very little from those which attend the displacements already described; we have the same bearing down pain, and sense of fulness about vulva and perineum, pains in loins and thighs, difficulty in voiding the contents of bladder and rectum, particularly the former, but this is not always the case. Dr Walsh mentions an instance in which no inconvenience was felt in voiding the urine, but there was great difficulty at stool. Legrand and others have met with cases in which no difficulty attended the evacuation either of bladder or bowels.

Diagnosis.—It must be evident that no decided reliance can be placed on the symptoms without a vaginal examination. Indeed, this is a *sine qua non* to enable us to form our diagnosis.

The os uteri is found in its normal place, and by running the finger along the anterior surface of the cervix, the chink or angle made at the point of curvature is felt, anterior to which the fundus is found impinging upon the neck of the bladder. The uterine sound enables us to distinguish the flexed uterus from all kinds of pelvic tumours, and thus gives certainty to our diagnosis.

We cannot speak too strongly of the great advantages derived from the use of this instrument in the elucidation of uterine diseases. If Dr Simpson should never add another improvement to medicine, the invention of the sound would entitle him to the best thanks of the profession. By the sound we can now form correct notions of a large class of important affections, which were otherwise involved in obscurity and uncertainty.

Before Dr Simpson made his invention known, I was in the habit of employing in the examination of uterine diseases an elastic bougie made upon a steel wire; but this was vastly inferior to the simple and ingenious instrument of Dr Simpson.

The speculum vaginæ is not required in the investigation or diagnosis of these misplacements of the uterus; the "*touch*" with the use of the "*sound*" being quite sufficient. In flexions of the uterus the speculum will be apt to mislead. We can only bring the os uteri into view, and as this retains nearly its natural position, we would have no reason to apprehend misplacement. Again, the os uteri being generally indurated, red, and even ulcerated, in these abnormal positions, if we use the speculum alone, we would be apt to conclude that the tumefaction, redness, and ulcerations were the sole cause of the patient's sufferings. I allude to this point, as I have known several mistakes to arise from the use of the instrument in

such cases. This could not possibly have occurred had the examination been made with the finger.

Treatment.—The treatment of the four misplacements under consideration may be divided into two parts: I. The Medical. II. The Mechanical.

I. MEDICAL TREATMENT.—It must appear self-evident to every person, that either when enlargement of the uterus or inflammation exists, these conditions must be removed before we can expect to cure the patient.

It may be said that the enlargement and inflammation are the results of the malposition, and not the cause; consequently it would be more rational to replace the uterus, and afterwards treat the effects. In answer to this, I would submit the following observations:—*1st*, The relation here assumed does not always exist; indeed, I think that it very rarely happens. *2dly*, Though you *can* replace the uterus, yet it will very soon relapse into its abnormal position, unless the enlarged condition be removed. *3dly*, The uterus frequently *cannot* be restored to its proper place until it has been reduced in size. *4thly*, By removing congestion and hypertrophy, the organ will very frequently either spontaneously resume its natural site, or pregnancy occurs, and subsequent to parturition no relapse takes place. *5thly*, When you have reduced the hypertrophied condition, if the uterus does not unassisted take its normal situation, manual interference will then be more successful. Hence, in a practical point of view, it is of very little importance whether congestion, inflammation, or enlargement has preceded or followed the displacement. Our treatment ought in either case to be conducted on the same principle; namely, the employment of means to remove the results of congestion and inflammatory action, previous to any effort being made to restore the organ to its normal position.

Judging from my own experience, and from the statements of several authors, especially of Boivin and Dugés, I have little hesitation in affirming, that when judicious means are used, in conformity with the principle here adverted to, few cases will require mechanical assistance.

The following is an outline of the treatment which I have found most useful:—

1. Confinement to the recumbent position, either on a sofa or mattress. Rest I conceive to be a matter of the greatest importance. Exercise always causes, not only an aggravation of the patient's sufferings, but I am convinced of the affection also, at least very essentially protracts the recovery of the patient. A consideration of the abnormal position of the uterus must render obvious the truth of these observations.

2. Regular condition of the bowels; this requires strict attention. In retroversion and retroflexion, constipation is particularly injurious. The impacted feculent matter in the large intestines must by

its pressure increase the extent of the misplacement, and as these hardened matters make their exit slowly through the rectum, a very considerable amount of irritation and congestion must be produced both in the rectum and uterus; in the former it is made evident by the large quantity of coagulated mucus (not unlike macerated macaroni) which is voided at stool; besides this, the patient's sufferings are greatly augmented.

The laxative which I have found most useful is either olive oil, or an electuary composed of sulphur, cream of tartar, and molasses. Of the former I order a large table-spoonful night and morning, if the oil be free from rancidity—it not only keeps the bowels easy, but it also improves the general health. In several cases the advantage of this over many other laxatives was most striking. Enemata generally prove injurious. I have not employed any thing of this nature for a considerable period.

3. Local depletion is of great use, particularly when there is much tenderness of the uterus, or severe pains about loins and thighs; in such cases local bleeding is almost indispensable. The application of a few leeches to vulva, vagina, or os uteri, once or twice each week, will prove most beneficial. Cupping the loins is also an excellent remedy.

4. *Mercury*.—When the uterus is much enlarged and indurated, my chief reliance has been on mercurials.

I have seldom found these fail when properly employed. At one time I prescribed small doses either of the mercurial pill, or of hydrargyrum c. creta, morning and evening, until the gums became tender. Since the iodide of mercury has come into general use, I prefer it to any other preparation. I order it in half grain doses every eight hours, made into a pill with conserve of roses and extract of hyoscyamus.

In my hands, its action has proved most beneficial in reducing the size of the uterus. Salivation has sometimes resulted from its use, and oftentimes it has produced considerable intestinal irritation, but this inconvenience was obviated by diminishing the dose. After the mercurial action has been established, I have found much benefit from the hydriodate of potass, given in combination with some mild vegetable tonic, such as colomba or cascarilla.

By this method of treatment the enlargement and inflammatory action of the uterus rapidly subsides, and the organ resumes (in many cases spontaneously) its natural position,—and if no adhesions have been formed, a little manual assistance is all which is necessary for its replacement. Often when the uterus becomes reduced in size, the patient ceases to experience any unpleasant sensation or inconvenience,—the symptoms and complaints disappear in proportion, as the uterus decreases in size. This leads me to believe, that if the uterus were merely displaced, and not augmented in size, the patient would not feel any great inconvenience from the accident; at least I am certain, not to the same extent as she must do when

hypertrophy exists. Not having met with any case uncombined with hypertrophy, I confess myself unqualified to discuss the subject any further than to repeat the remark, that in several cases of retroversion in which the organ was greatly hypertrophied, and in which the patient suffered exceedingly, when the hypertrophy was removed, all the symptoms vanished,—and though the uterus still remained in its abnormal situation, yet the patient was able not only to go about without any inconvenience, but had no return of those feelings which previously had rendered her unable to take any exercise. Now, we would think if the malposition was the cause of the inflammatory action, and consequent hypertrophy, that in such cases a relapse of this condition would have occurred. So far as my experience extends, I have not observed any such thing taking place.

Improvement of the general health I hold to be of the utmost importance in the treatment of displacements of the uterus. Profuse menstruation must be checked,—the action of the stomach, liver, kidneys, and skin promoted; sponging the surface with a solution of salt, and the tepid bath medicated with salt and mustard do much good. If vagina be much relaxed, the alum hip bath is useful. In a few words, we should have recourse to every means that is calculated to improve the tone of the general system,—this ought to be kept steadily in mind in the treatment of every form of uterine disease, as well as in those at present under consideration.

In all local diseases, especially of long duration, occurring in impaired constitutions, whether that impairment has preceded or followed the local affection, no local treatment will succeed if we do not at the same time improve the general health.

II. MECHANICAL TREATMENT.—This may be divided into manual and instrumental. Several authors recommend the uterus to be replaced by the hand introduced into vagina and rectum; for a description of these manipulations, I refer you to the works of Ashwell, Boivin and Dugés, &c.—This mode of treatment may be useful when the displacement is recent, and uncomplicated with hypertrophy, especially if the displacement has occurred immediately after parturition.

With regard to the use of instruments, I can say little from my own experience; various kinds of pessaries have been proposed—none probably so ingenious as those invented by Dr Simpson.

These instruments, *especially the latter, may be serviceable* in retroversion; but I doubt their efficacy in cases of anteversion, and the flexions of the uterus. If inflammatory action exist, I would be afraid that the presence of these instruments would excite this process, and thus aggravate the case. It is but justice, however, to mention, that Desormeaux and others consider that the irritation caused by the use of the common pessaries tends very much to cure the enlargement of the uterus, by increasing the action of the absorbents. If there be any truth in this opinion, Dr Simpson's in-

struments must prove useful in two ways; that is to say, by removing hypertrophy, and maintaining the uterus in its proper place. If no bad consequences result from the employment of the style, and if it retain the uterus in its proper position until enlargement be removed, I consider that the instrument will be a most valuable acquisition in the treatment of retroversion, especially as the patient will be thus enabled to avoid the confinement which is otherwise indispensable—and which tends more or less to impair the general health in young females.

The sponge pessary I have often introduced, after the uterus had regained its normal size:—I have never found its use to be required beyond a few weeks; in some cases it was discontinued in a few days, in consequence of the irritation which it caused.

Probably my good fortune may have been owing to the mildness of the cases which have come under my care. I would remark, however, that in more than one instance, where mechanical means had been previously employed without success, the medical plan of treatment which I have recommended proved most effectual.

I may mention, that Boivin and Dugés recommend antiphlogistic treatment in all cases of flexion and anteversion of long standing. Levret, Désormeaux, Lachapelle, also treat these displacements by antiphlogistics, the opinion of these authors being that chronic metritis always exists.

I am happy to add the testimony of Dr Beatty of Dublin, in confirmation of the mode of treatment which I have advocated. Since I commenced to write this paper, I have read with much pleasure his excellent article on retroflexion of the uterus (in the *Dublin Quarterly Journal*.) He narrates two cases successfully cured by treatment conducted on the same principles, and by similar remedies as those which I have mentioned.¹

In submitting the following cases to the consideration of the reader, I would observe that I do not wish him to regard them as *proofs* of the curative power of the mode of treatment adopted, but simply as the record of the effects which have followed that treatment, not only in the few cases which I intend to quote, but in a very considerable number of the same character which I have had under my care. The cure of any disease is perhaps as complex as its cause, that is to say, depends on the conjoint action of several conditions or agencies. Hence it must be always a difficult question in medical reasonings to determine the exact share or

¹ Since this paper was read, several valuable communications have appeared on the same subject in the various Medical Journals. Dr P. Smith (*the Obstetric Record*, p. 35), Dr Hensley (*Provincial and Surgical Journal*, Jan. 1848), Dr Simpson (*Dublin Journal*, May 1848), and still more recently Mr T. S. Lee (*Medical Gazette*). Dr Simpson has appended to his valuable contribution an excellent bibliographical note, which must prove very useful to those who feel interested in these affections.

relation which any mode of treatment holds in the cure of disease; consequently, cases in which recovery occurs are more to be regarded as *illustrations* than as *proofs* of the effects of any plan of treatment. To view them in a stronger light will lead to error; indeed, cases may thus become most detrimental to the progress of medical science. On the other hand, I would beg to dissent from that scepticism which denies all relation between medicinal agents and the cure of disease. When we find affections yielding almost invariably under the employment of certain remedies, I contend that it is irrational to deny that these agents have no share in effecting the cure, because similar cases so treated have terminated fatally, or got better under different means, or without any at all. It surely does not follow, that the results of our treatment ought not to guide us in the management of disease, because we cannot explain to the satisfaction of one who shuts himself up in the dark folds of his cold scepticism, how certain agents occasionally fail to effect a cure in diseases, in the treatment of which a successful result generally follows their use. Such a mode of reasoning is still more fatal to the advancement of medicine than the former. A medium course is the best. The combination of several circumstances may be necessary to effect a cure in every case. Some of the conditions necessary to a successful result we too often overlook,—others are beyond our powers to appreciate, and some we cannot render available. Thus, the constitution of the patients, their age, the mental and physical circumstances under which they have been, and are placed,—the stage of the disease, its complications, &c. &c.,—all tend to influence the result of any medicinal treatment. Thus the process of cure, as I have already observed, is as complex as the cause from which the disease originated; consequently we err most seriously when we ascribe the cure to our "*physic*" alone. I have made these remarks, so that I cannot be considered as demanding a greater amount of consideration for the cases which I shall now quote, than that to which they are justly entitled. I place them before the reader as a faithful record of certain occurrences following a mode of treatment, in a class of cases which some talented medical men seem to consider unmanageable by medicinal agents, and to be cured principally, or alone, by instrumental means.

CASE I.—*Retroversion of Eighteen Months' duration—Cure by Rest—Mercurials—Leeches.*

July 10, 1841. Mrs A—, of a sickly emaciated appearance, mother of three children—complains of general derangement of health, weakness, pain of back referred to the sacrum, dragging sensations about iliac regions, difficulty and pain in voiding the contents of the bowels and bladder. A thick cheesy discharge from vagina—bowels constipated—globus and other hysterical symptoms. Complaints commenced shortly after the birth of her last child, eighteen months ago. Had excellent recoveries after former deliveries, being able to resume her domestic duties on the second or third day after birth of child—on the last occasion had more than the usual amount of exertion in con-

sequence of a change of residence, which she had to effect on the third day after delivery.

On making a vaginal examination, the uterus was found retroverted, the fundus lying low down; it was painful when touched, and considerably enlarged.—*Habt. Hyd. c. cret. gr. iiss.*—*Extr. Hyoscy. gr. ij. ex form pil. t. d.*—*Admov. Hirud. vj. ad Memb. inter. Vagin.*—*Habt. ol. Olivar ʒj b. d.*—*Quiescat in lectu.*

This patient's gums became tender at the expiry of eight days, when the mercury was discontinued. The leeches were repeated, and she was ordered to use the following mixture three times each day:—*R. Hydriod. Potass. gr. iiss. Tinct. Cardamom. co., T. Hyoscyami, nigr. aa. ʒss. Infus. Colombæ, ʒij.*

When I examined her on the 20th August, the uterus had resumed its natural position and size, and was free from all tenderness—she had no complaint, and had regained flesh very considerably.

I might quote many cases very similar to the above, but this I deem would be unnecessary. The one now narrated affords a very fair specimen of the result of the plan of treatment which was adopted.

I may mention, however, that in some cases connected with dysmenorrhœa, and also with tumours in the posterior wall of the uterus, the organ in the former class of complications often relapsed into its displaced position until the dysmenorrhœa was cured, and in the latter class, viz. where tumours existed, the uterus, though replaced, generally relapsed. The first case of this kind which comes under my care, I shall make trial of the style pessary of Dr Simpson, in order to prevent, if possible, the repetition of the displacement.

CASE II.—*Retroflexion of Three Years' duration—Treatment by Local Depletion—Mercurials—Tonics—Cure—Subsequent Pregnancy.*

September 6, 1843. Mrs R—, æt. thirty years, complains of severe pain at lower part of back, pains through abdomen, dragging sensations about iliac and hypogastric regions, a feeling of heaviness about perineum, leucorrhœal discharge profuse. Her complaints are much aggravated by exercise—indeed, she is almost unable to walk; has much difficulty in making water, as also in defecation; stools contain a large quantity of coagulated mucus, not unlike maccaroni.

On making a vaginal examination, the uterus was found in a state of retroflexion, and much enlarged; the fundus pressing down on posterior part of perineum; the cervix and os much augmented in size, and turned somewhat forward to pubis; the os dilated, and its limbi hard, and covered with superficial abrasions. In consequence of the amount of hypertrophy which existed, the uterus could not be moved to any extent; in fact, it completely packed up the lower part of pelvis.

Her complaints commenced a few days after birth of last child (three years ago). She had walked from one bed-room to another on the second day after delivery, and, when in the act of walking, she found "something give way or fall down in the right side." This was followed by difficulty of evacuating the contents of bowels and bladder, and febrile symptoms. Was treated for "inflammation" at the time. Has not enjoyed good health since; suffers much from profuse menorrhagia at the menstrual period: it was in consequence of this that my attendance was requested.

The attack yielded to gallic acid. After this she was ordered *Hyd. c. cret. gr. iij. Extr. Hyoscyam. gr. ij. in form. pil. b. d. et Habt. ol. Olivar ʒij. om.*

nocte. Absolute rest was at same time enjoined. At the end of a week from my first examination of the case, not finding the tenderness yielding, she had eight ounces of blood taken from the loins by cupping; this was repeated twice at intervals of a few days. On 21st September the mercury was discontinued, in consequence of tenderness of gums. She then had the hydriodate of potass and Infus. Colombæ mixture. On the 10th November the uterus was found reduced nearly to its normal standard, but still retroflexed. She would not submit either to manual or instrumental interference for its adjustment, as all her complaints and uneasy feelings had disappeared.

She became pregnant a few weeks afterwards, and was safely delivered at the end of her full period. After delivery I caused her to keep her bed for the period of three weeks; a very necessary precaution to prevent relapses, and one which I always adopt. In the present instance no tendency to relapse occurred. I may mention that this patient has had two children since.

CASE III.—*Retroflexion of Long Duration—Mercurial Treatment—Fundus Uteri replaced by Hand—Cure.*

Sept. 11, 1846. Mrs S—, æt. thirty-two years; tall, and of dark complexion; complaints very varied—all of an hysterical character. She has pains every where, but fixed nowhere for any length of time. Appetite much impaired; some debility; want of sleep; pulse 78, weak; tongue furred; bowels regular; catamenia regular; a thick vaginal discharge exists. On a vaginal examination the os uteri was detected nearly in its proper position, but the fundus was lying between it and the rectum, in the form of a large tumour. At the point of junction of the neck with the body of the organ, the finger easily found the angle or chink at which the flexion had taken place. The os uteri felt indurated; and, by the aid of the speculum, several superficial abrasions and fissures were detected in its limbi.

About six years ago she had her last child. Two days after birth the infant was seized with convulsions; this caused her to rise frequently from her bed. She was then seized with what was called inflammation, since which she has never been well, but lately has become much worse, more nervous, dyspeptic, and annoyed with pains shooting through the pelvis. She has been for some time under tonic treatment for her weakness, but without any advantage. She was ordered to keep her bed, and to take—*Hydrarg. Iodid*, gr. ss.; *Ext. Conii. Ext. Hyocy.* aa. gr. i.; *Misce et fiat pil. omn. mane et nocte sumend.*

Her pills had to be suspended on the 18th in consequence of diarrhœa, but were resumed on the 20th, and continued, with occasional intermissions of a few days, to 20th November, when the following note of her case was recorded:—Is greatly better in every respect; uterus much reduced in size, and easily moved in every direction. She was placed on her elbows and knees; a large rectum bougie, with a piece of sponge tied to its end, and covered with oil-silk, was well oiled and introduced into rectum, in the same manner as used by Dr Ashwell,¹ the fingers of the right hand at same time being introduced into vagina. By pressing the os uteri backwards with these fingers, and the fundus uteri forcibly upwards by the bougie, the organ was with some difficulty placed in its normal position. A piece of sponge, covered with oil-silk, was introduced into posterior part of vagina. This was discontinued at the end of two weeks. No relapse has taken place. The patient now enjoys the best of health.

CASE IV.—*Retroflexion—Great Enlargement of Uterus—Relief—Relapse.*

June 10, 1843. Mrs G—, æt. forty-five years, mother of a large family, has had profuse menorrhagia during the last two days. She has had similar attacks during the last two years and a half. The discharge on

¹ On Diseases of Women, 2d edit. p. 618.

the present occasion was restrained by gallic acid and cold applications. On inquiry, I found that her health had been much impaired from the birth of her last child; a few hours after the delivery of which, in consequence of the clothes of another child having caught fire, she arose from her bed suddenly, and was seized with inflammation the day afterwards. From this time she never fairly recovered. Has been much troubled with pains through the pelvis and lower part of the back, with difficulty of voiding urine; and she suffers much when at stool. Her menstruation is most painful and profuse. On making a vaginal examination, the uterus was found to be not only retroflexed, but enormously enlarged. The os uteri covered with deep fissures. She was ordered to keep her bed, to take the iodide of mercury, and to use the *Krameria triandra* as a tonic. At the end of two months her complaints had completely subsided. The uterus reduced to its natural size, but resisted every effort to restore it to its natural position. She felt no inconvenience for several months. In the end of August she had a severe attack of dysentery, after the subsidence of which her old complaints returned; the uterus becoming much enlarged and painful. She was again put under the previous treatment, and in a few weeks the size of the organ was reduced, but no replacement could be effected. She experienced no inconvenience from the malposition, and is at the present time (November 1846) in the enjoyment of good health.

I have met with several instances, besides that of Mrs G—, where no inconvenience was felt by the patient when the enlargement of the uterus was removed; the organ still remaining flexed. In the present case, the want of success attending the attempts to restore the organ to its natural position, arose from adhesions having been formed between it and the neighbouring parts.

CASE V.—*Retroflexion of Long Standing—Mercurial Treatment—Pregnancy—Cure.*

Nov. 23, 1846. Mrs J—, æt. twenty-eight years, of low stature, dark complexion, said to have been labouring under uterine disease during the last four years. Her complaints were very similar to those so often enumerated. On making a vaginal examination, the uterus was found to be retroflexed, and much augmented in size, so much so that it could scarcely be moved in vagina. Her general health is much impaired. She has undergone much treatment of various kinds. I ordered her to keep constantly either her bed or the sofa, to maintain an easy state of bowels by olive oil, and ordered her the iodide of mercury and vegetable tonics. These remedies were followed by great improvement of the general health, subsidence of her complaints, and reduction in the size of the uterus. Early in January 1847, the organ could be easily moved about the vagina, and was free from pain, but continued retroverted. I wished to endeavour to restore the fundus to its place, but the patient informed me that the menstrual period had been passed for a fortnight. Under these circumstances, pregnancy being probable, no interference was made. Pregnancy had occurred, and she was safely delivered of a fine female child, at full period. I caused her to keep her bed a full month after delivery. No relapse has taken place.

In this instance, as well as in Case II., the cure may be ascribed to pregnancy. I have seen another case lately, in which a similar result has occurred. The enlarged condition of the uterus seems to prevent impregnation, or at least gestation; but by the removal of the diseased condition, the organ seems quite capable of performing its functions, independently of its flexion. Abortion, however,

frequently takes place, and in one of these cases the woman was often threatened with this occurrence.

CASE VI.—*Anteversion—Mercurial Treatment—Rest—Cure.*

On the 20th May 1842, I was requested to visit Mrs W—, æt. twenty-six years, in consequence of her inability to void urine. On inquiry, I found that about eight months since her first child was born; she had a good recovery, was able to leave her bed on third day after delivery; soon after she experienced difficulty in voiding urine, pains shooting through from pubis to back; these she ascribed to jumping suddenly from a chair. The pains and dysuria have continued to annoy her much; indeed the latter has gradually increased in severity, and now there is complete retention. Her general health is much impaired.

On instituting a vaginal examination, the fundus uteri was found lying behind the symphysis pubis, the os tilted up to the promontory of the sacrum. With considerable difficulty it was brought downwards (by introducing the two first fingers of the left hand into the rectum), and the urine withdrawn by the catheter. The uterus was then completely restored to its proper position. On the 22d, I was again sent for under the same circumstances. Similar manipulations were adopted with the same result; but in order to maintain the uterus in its normal situation a sponge pessary was introduced. This could not be worn; a ring one was next applied, but it had no effect in preventing the displacement. (June 1st), She was now ordered to keep her bed, and to take hydrarg. c. cret. gr. ij. every six hours. Care was also taken to prevent accumulation of urine. On the 28th the uterus was reduced in size and had resumed its proper place, and no relapse afterwards took place.

In this case, the advantage of removing the congested condition of the uterus was very obvious. Every thing previously done was entirely useless. If I had placed her under the mercurial treatment earlier, she would have been saved much pain and annoyance; but I was led, from the ease with which the organ could be replaced, to think that the pessaries would be sufficient to retain it, and that it would not be necessary to subject her to confinement to bed, and the risks which must always more or less attend a mercurial course. I will never allow such considerations again to influence the treatment.

CASE VII.—*Anteflexion—Enlargement—Mercurial Treatment—Relief—Pregnancy—Cure.*

May 2, 1846, Mrs M'L—, æt. twenty-seven years, mother of one child, complains of difficulty of voiding urine, pain in hypogastrium and left iliac fossa, a sense of weight and fulness about the pubis, and pains shooting down the thighs; bowels regular; catamenia regular, sometimes scanty, and again profuse; complaints commenced last January, when she had an abortion, followed by an acute attack of metritis; before marriage she suffered much from *dysmenorrhœa membranosa*.

On making a vaginal examination, the os uteri was found in its usual position, (but low down in the vagina), the limbi were large and indurated; the fundus was lying behind the symphysis pubis, large and firm, painful to the touch: the point at which the organ was flexed upon itself was at the junction of the cervix with the body.

She was ordered to maintain the recumbent position, and to take half a grain

of iodide of mercury three times daily, to have six leeches applied above the pubis once weekly. A month after the commencement of this treatment the uterus was very much reduced in size, but still remained flexed, though to a much less degree than before. All her complaints were gone; indeed she considered herself quite well, and would not submit to any manual treatment. In a few months she became pregnant. During gestation she had frequent threatenings of abortion, indeed was obliged to keep her bed the greater portion of the time; but with care she reached her full period, and was safely delivered of a healthy female child. She had no relapse of the displacement, and has enjoyed excellent health since her confinement.

Pregnancy in this instance had the effect of restoring the uterus to its normal position; the treatment, however, bringing the uterus into a condition capable of performing its functions.

ARTICLE III.—*Case of Fibro-Cartilaginous Tumour of the Femur.*
By KELBURNE KING, M.D., Port-Glasgow.

ABOUT the end of last March, J. A., aged seventy-three, consulted me regarding a feeling of pain and weakness of the upper part of the left thigh, which had continued for about a fortnight, and though unattended by any swelling or other outward appearance, rendered him unable to lean his weight on the affected limb, and consequently prevented him from taking out-door exercise. While he sat he experienced no inconvenience, but on attempting to walk, he felt as if the movement of the limb was arrested by a painful feeling in the upper and inner part of the thigh, corresponding to the insertion of the adductor longus muscle. This pain was very severe after going to bed, but was not then confined so much to one spot, being referred sometimes to the knee, sometimes to the leg, and sometimes affecting the opposite limb. It often lasted during the greater part of the night, preventing him from sleeping till towards morning. Previous to this attack, he had enjoyed good general health, with the exception of occasional slight derangement of the liver and stomach. At the commencement of last spring, he had for a short time complained of similar pain in the same part—he had taken little notice of it, and it had gradually disappeared.

On examination, nothing abnormal could be detected. There was no swelling of the part; pressure produced no pain. The thighs were muscular, the affected one as much so as the sound. In short, there was neither pain on pressure, nor swelling at any part of the thigh or hip. His complexion, however, was bad, having a dirty yellowish aspect; his tongue was red at the point and foul behind—his pulse full and rapid, usually about ninety. His appetite was but little affected, and, though his bowels inclined to be costive, he asserted that his health was perfectly good if he could only get the use of his leg.

Under the impression that his complaints were referrible to rheumatism, accompanied with derangement of the chylo-poietic viscera, stimulating applications were made to the thigh, and he was put on a gently alterative course of medicine. At first some benefit seemed to be derived, and in the middle of April he was so much better as to be able on one occasion to take a short walk in his garden. The improvement, however, was not permanent. He soon began to fall off perceptibly in flesh and strength, his pulse increased in rapidity, his look became more anxious, and his sufferings at night more severe, so that now he obtained rest only by means of opiates. Dr Watson of Glasgow saw him along with me towards the end of April; but at that time and for some weeks after, the erratic nature of the pains, and the absence of any appearance of disease in the femur (though carefully looked for), led us to consider his complaint to be of the nature of nervous rheumatism. This, together with depressing circum-

stances in his private affairs, and his advanced period of life, seemed to us sufficient to account for the general giving way of his health. After this, colchicum, hydriodate of potash, cod-liver oil, &c., were successively employed, but without procuring relief. The pain increased in severity, occurred frequently in violent paroxysms, and was not limited to the place originally most constantly complained of, being greatest sometimes at the knee and sometimes as if shooting from the back. He became totally unable to put the foot to the ground, and was constantly confined to bed. On the 15th May a careful examination of the limb was made by Dr Watson, but with no better result than before. On the 18th he suffered a succession of the severest paroxysms he had yet experienced. He could not say at what point the pain was greatest, referring it indifferently to the knee, to the hip, and to the shaft of the bone. Relief was obtained only after large doses of opium. During one of the paroxysms he declared, that he felt as if a shot had passed up the affected limb, and shortly after repeated the same remark with exclamations of alarm. The next day the pain had subsided. On visiting him, I found that his attention had been attracted by a swelling at the upper and outer part of the thigh. This, on examination, proved to be a hard osseous tumour, about the size of a man's fist, situated below and in front of the great trochanter. The limb was considerably shortened, the knee bent in beneath the other, the foot inverted; altogether very much in the position caused by dislocation on the dorsum ilii. Any attempt to move the limb caused him extreme pain, and crepitation of an indistinct and, as it were, muffled character, could be detected by the hand placed on the top of the tumour when such motion was made.

For some time previous to this remarkable change in the symptoms, I had been led to suspect the existence of deep-seated, most probably malignant, disease of some organ—which I could not determine,—by the anxious expression and saffron colour of his countenance, the constantly excited pulse, the quick progress of emaciation, and the rapid falling off of his strength; I felt convinced, after examining the tumour, that it was caused by fracture of the femur, and consisted of the upper end of the lower fragment drawn upwards and outwards by the action of the muscles. The question then came to be, how did this fracture occur? It was certainly not produced by external violence, as he had not even been out of bed for some days. It was necessary, therefore, to look for an explanation to internal disease, and, under all the circumstances, it appeared to me, that the pain which had so long existed, and whose precise seat and cause had been so obscure, had proceeded from disease existing in the medullary canal of the femur, which, at first confined, had gradually overcome the resistance offered by the bone, and now by breaking through the shaft caused fracture. This was the opinion of Dr J. R. Spiers of Greenock also, who saw the case with me the same evening. We did not think it desirable to disturb the patient by attempting to extend the limb; and our attention was after this directed to sustaining his strength by cordials, regulating his bowels, and preventing the formation of bed-sores. There was never after any violent pain. He died on the 1st June.

Two days after death I made, with the assistance of Dr Reid of this place, a post-mortem examination of the thigh. An incision was made on its outer side, from the great trochanter to the knee. The muscles, though soft, were healthy. There was no extravasation of blood, nor any appearance of disease until the bone was reached. This was found fractured about two inches below the small trochanter, in an oblique direction from behind, forward and downward. The edges of the fractured surfaces were thin, rounded off at the anterior aspect of the bone, and brittle. The diameter at the seat of fracture was slightly increased, as if expanded from within. From the cavity of each fragment protruded a firm fleshy mass of a reddish colour. On following these they were found to be connected, both above and below, to the cancellated structure of the bone, by a firm pedicle of a fibro-cartilaginous nature. Their opposite surfaces corresponded exactly, leaving no doubt that originally they had constituted one mass, which had been divided by the weight of the limb;

the moment of separation being, most probably, that when he exclaimed that a shot had passed upwards through the limb. On placing the broken surfaces in apposition, a circular portion was deficient from the anterior part of the shaft, as if eroded by the pressure of the tumour; and the anterior aspect generally was thinned away more completely than the posterior.

On cutting into the tumours they were found of a greyish colour, having the appearance and consistence of cartilage, except towards their opposing surfaces, where they were of a red colour, and somewhat softened. This, however, in my opinion, was produced by the rupture, and did not depend on malignant action. The size of each was about that of a walnut. It should be mentioned, that though surrounded by bone, they were not attached to it except by the pedicles mentioned before. There was no attempt at production of new bone, or reparation, in any shape; nor was there the slightest appearance of inflammation, suppuration, or other consequence of injury.

We were not permitted to examine any other part of the body.

Remarks.—The foregoing case appears to me interesting in various points of view. In the first place, it differed from ordinary cases of osteo-sarcoma in the absence of swelling. In this disease the bone usually undergoes a species of degeneration and disorganization, becoming expanded in its tissue, and softened to various degrees of consistence from cartilage to gelatinous substance. Here, however, it appeared rather as if the bone yielded to the pressure of a growth foreign to itself, taking its origin, indeed, within its medullary sheath, but still producing upon it no effect except the gradual absorption caused by its continued enlargement. It did not appear from the post-mortem examination, that any morbid changes had taken place in the osseous texture. It was neither softened nor expanded by disease; it was simply thinned away, and forced slightly outwards by mechanical pressure applied from its centre.

Again, there resulted from this peculiarity in the nature of the case, a striking obscurity in the symptoms. Great pain, no doubt, was referred to the part, as is usual in osteo-sarcoma; but it was not constant in the same spot, nor even in the same limb, so that its erratic nature, increase on getting warm in bed, and the previous history of the case, combined with the absence of swelling of the bone, pointed rather to chronic rheumatism than to local lesion.

I would direct attention also to the rapid progress which was made in the latter stage of the complaint. A year before I saw him he had experienced pain and weakness of the thigh; but he paid little regard to it, and indeed felt little or no inconvenience from it till two months before his death. During the first period of its duration, it must have been dormant and inactive; but after that time its rapid progress, and the severity of the symptoms, indicate an amount of activity characteristic more of malignant than of benign disease. It is to be regretted that the tumour being incautiously put into spirits, advantage could not be taken of the powers of the microscope to throw light on this subject. Its physical characters, however, as evident to the senses, were those of the simple fibro-cartilaginous growth.

I have only to remark further, and it is my principal reason for

putting this case upon record, that had the nature of the disease been known at an earlier period, the treatment adopted would have been very different. I will not stop now to inquire, whether in that case it might have been got rid of by the use of the trephine and cutting pliers, or whether the more serious measure of amputation would have been required. Had the expansion been so great as to have been felt when I first saw him, or soon after, I think a successful attempt might have been made with the former instruments; so much is clear, that the treatment pursued could not exercise any beneficial effect on the disease which actually existed, whilst, had we possessed the means of making an accurate diagnosis, a chance of relief would have been afforded to the patient.

ARTICLE IV.—*Observations on the Employment of Chloroform in Dental Surgery.* BY FRANCIS BRODIE IMLACH, Dentist, L.R.C.S.E.

(Read before the Medico-Chirurgical Society, Edinburgh, July 5, 1848).

EARLY in November last, I extracted the first tooth ever drawn from a patient placed under the anæsthetic influence of chloroform. Since that period I have, in my practice as a dentist, used it now above 300 times, and without one single accident or drawback of any kind. And I have at this time been induced to lay before the Society the results of my experience in regard to its use, from having heard repeated accounts of the alleged total failure of this agent in dental surgery; and that sometimes in the hands of men of extensive practice.

Let me merely premise, that I have tried chloroform manufactured by different chemists; but that supplied by Messrs Duncan and Flockhart is what I have found most pure, and most easy of inhalation. It is therefore that which I now constantly use.

I do not know if I have seen all the instruments that have been invented for the purpose of administering chloroform; but I have seen a very great number indeed, many of them very ingeniously contrived, chiefly with the object of preventing the loss by evaporation of chloroform; but in general they are so arranged, as to fit over the nose and mouth of the patient, and prevent his breathing a particle of air before it has passed through the apparatus—a point in which, I believe, there rests an insuperable objection to the use of all ordinary instruments; and I have not seen a single one that I could be induced to use in preference to the method first recommended by Professor Simpson, namely, that of a handkerchief.

It has the recommendation of being perfectly simple, convenient and easy of application, does not frighten the patient; and, what is best, it most effectually combines every requisite for perfect administration.

Some dentists among us are in the habit of placing two or three

plies of cotton wadding within the folds of the handkerchief, for the purpose of preventing the chloroform from evaporating so rapidly as it otherwise would do, and keeping what they do employ concentrated in one small spot. Certainly they may, in this way, succeed in rendering their patient insensible with a minimum quantity, provided that he is obedient enough to breathe in the exact way that is desired. If, however, the patient is not very manageable, he will not rapidly become anæsthetic by breathing from so small a surface; and then the operator, finding it difficult enough to make him insensible, is induced to place the handkerchief nearer and nearer the face, and perhaps close upon it. Then, here is a sufficient reason why the patient should begin to show what are called bad symptoms, not because the chloroform is bad, or the patient a difficult subject, but because he feels a great sense of choking, and very naturally tries to rid himself of that sensation by rolling the head backwards and forwards, speaking, throwing up the arms, trying to get from off the chair, or, in fact, by any other means that his half-drowsy sensibility can stimulate him to. In a very nervous patient, the mere touching him with the handkerchief whilst he is inhaling, is apt to excite, by recalling his sensibility to what is being done; and, as the first stage of chloroformization is a state of excitement, great care requires to be taken so as to get a patient of this sort past this stage as quickly as possible; and, having him safely past it, you may then touch him freely without any danger.

Perhaps the Society will allow me to enumerate a few simple and easy rules which I have always found useful, and to which I confidently attribute some of the success which has always attended the exhibition of chloroform in my hands.

Firstly. I settle the patient in an easy and comfortable position, with the head supported by some firm object. I am also in the habit of requesting him to close his eyes, in order to avoid the irritation of the chloroform vapour, and to prevent any accidental mental or physical excitement, as from the sight of an amusing object, or the glare of too bright light. Of course, all noise of every kind is strictly prohibited during the inhalation, and I think it advisable to warn him against being alarmed by extraordinary noises, as ringing in the head, or by flashes of light before the eyes, or a feeling of giddiness, or vertigo.

Secondly. As to the dose. I am always in the habit of pouring a large quantity upon the handkerchief, and diffusing it over a surface larger than will cover the nose and mouth. The quantity I never measure, as I judge by the effects and not the dose; but I believe I seldom begin with pouring, at first, less than three or four drachms upon the handkerchief. In fact, I pour upon it a quantity sufficient to moisten completely the required surface. Many of the supposed bad effects, doubtlessly, result from giving a small, imperfect, and consequently mere exciting, dose.

Thirdly. The mode of administration. The chloroform is poured

upon a thin white cotton handkerchief. An old one answers the purpose remarkably well. This I hold at first at the distance of two or three inches from the patient's mouth, and allow him thus to take two or three deep inhalations. I then approximate it more closely to the face, but never bring it into close contact. In this position the handkerchief is continued till the anæsthesia supervenes. In judging of this point, I depend upon no single circumstance, and the indications are of such a varying nature as cannot be described, but are easily perceived after some experience in the practice.

Fourthly. The moment the handkerchief is removed, I apply the instrument (kept ready in my hand), and at once proceed to extract the tooth. If several teeth are to be extracted or punched, or if I require to change the instrument in use, it may be necessary to recommence the inhalation for a short time. This readiness at the proper time, and the rapid and immediate extraction, I consider the chief secret of success.

Fifthly. The great mass of patients, on becoming insensible, have their jaws so clenched, that there is no possibility of opening the mouth, and the operator keeps working away, tugging at the under jaw, or pressing upon the angle of the jaw externally with his knuckles, trying to force open the mouth. This, again, is easily remedied, by the very simple method of never allowing the patient to close his jaws at all, by placing a small gag of ivory or gutta percha between the teeth, before administering the chloroform. No patient objects to it, and it causes him no inconvenience whatever.

A patient of my own, who is in London at present, went a week or two ago to one of the first dentists there, to have three or four stumps extracted. He placed nothing between her teeth, had her insensible three or four times, but never succeeded in getting out any of the stumps, as sensibility always returned before the jaws could be forced open. This is a circumstance that takes place day after day, and is the cause of many dentists not using it at all in their operations. Now, such a palpable oversight as this is set down as one of the difficulties of using chloroform, when to me it seems to be one of its principal advantages; namely, that you can have your patient's mouth kept open whilst you extract one, two, or half a dozen teeth if required, and then you slip out the gag, and the patient is left to recover his sensibility, without your having almost required to touch him except in seizing the tooth. Certainly, the patient can be made to inspire chloroform till the muscles of the jaw become relaxed; but then, this causes him to inspire a vast deal more chloroform than what is requisite for keeping him insensible whilst extracting the tooth, and this plan also requires a proportionately longer time before he recovers his sensibility. I have seen a patient begin to inspire, become insensible, have the tooth extracted, and return to perfect consciousness in little more than a minute.

The other day I was called out, along with Professor Simpson, to see a patient, a young lady, who, we were told, could not be made

insensible previously to having a tooth extracted, and would not submit to the operation without anaesthesia. The gentleman who had seen her before we were called in, had been half an hour trying to put her over, and had not succeeded. Professor Simpson poured some chloroform upon the handkerchief, held it to her face, but, mark what she did to prevent herself becoming insensible—she first kept in her breath as long as she could, then took short inhalations, so as to take as little fresh air as possible into her lungs, and, whenever she felt herself pressed for want of air, she suddenly tossed her head aside, pulled away the handkerchief, and immediately took two or three full inhalations, so as to refresh herself, and fill the lungs with pure air; and then she was willing to allow the handkerchief to be replaced before the face. Dr Simpson at once discovered the trick; and the next time that she attempted to repeat the same actions, he prevented her pulling the handkerchief down, and followed her face with it as she turned it from side to side, thus compelling her to inhale the chloroform whilst appealing to her mother, who was present, to be allowed to speak for a moment or two. Of course she was allowed to speak as much as she pleased; but it was only for a few seconds that she did so, as the vapour inhaled during the act of speaking, was sufficient to affect her rapidly, and in the course of a minute or two at most she was completely insensible, and the tooth taken out.

Now, I required to see this young lady again professionally, and she attempted the same trick with me; but I acted as before, held her against her inclination firmly in the chair for a few seconds, and she was put over as rapidly as formerly. In a case of this sort, the patient inhales a great deal more than is requisite to make an obedient patient insensible, and on this account it is always well to pour an excess of the chloroform upon the handkerchief. I have remarked some of my patients become sick upon recovering from a dose of chloroform; but almost all those cases arose from an overloaded stomach at the time of the operation.

I have also seen them sick where, on inquiry, I found that they had tasted nothing for four-and-twenty hours previous; but then the whole system was deranged, from the fasting, irritation, and want of sleep consequent on a severe attack of toothache.

I have seen a few cases, perhaps three or four, in which the patients have been slightly hysterical; but, on the other hand, I have not had a single patient faint in my house after any operation performed under its influence, whilst before, I often had them lying on my sofa in this state, or in a state approaching to it. Besides all this, the patients used to come into the dentist's room with their faces pale, their lips quivering, their mouths parched, and with every symptom of terror depicted on their countenances. Now, on the other hand, they seem to think it a good joke to have a tooth taken out; and many a tooth I have taken out because the patient could get it done without any pain. Again, I have seen the circumstance

of the respiration ceasing for a few seconds, happen in several instances, and I now always wait till it recommences, and allow the patient to take again a few inhalations before operating.

There is another class of patients who are apt to display symptoms that might cause a little difficulty to the operator, in so far as to render him not able to distinguish whether they are insensible or not—I mean sleep-walkers. I have administered it to several of them, and in most of these cases the patients whilst insensible had the same appearances, and the same remarkable character of gesture and countenance, as are commonly attributed to sleep-walkers. I always find that the eyes of somnambulists gradually open as the chloroform takes effect, and that, when they are insensible, the eyes are fixed and wide open. Generally the patient attempts to get up, and if you are not a little dexterous, and catch hold of the tooth without touching, and consequently irritating, him, there is a great probability that the head, body, or arms, will be moved in such a way as to prevent you from operating. Now, in cases of this sort, do not touch the patient, or try to steady his head, for, if you do, you assuredly will not succeed in your object; let him alone, and in the course of a very few seconds he will become quiet again. Then seize the opportunity to place the handkerchief nearer his mouth, so as to make him a little deeper, and then, by avoiding the previous error, there is little doubt but that he will remain quiet long enough to have his tooth extracted.

I may here state, in addition, that I believe an over-dose of chloroform, and the consequent perfect relaxation of all the muscles of the mouth and tongue, is a great impediment to easy operating. The tongue in this condition doubles up like pulp, or a bladder, around the finger or instrument, and prevents the surgeon seeing and seizing the tooth.

Inattention to the great variety of temperaments that are met with, and the neglect of the little niceties of manipulation requisite for what may be called the polite administration of chloroform, is one great reason why so many dentists fail in managing their patients easily and successfully. I have often heard of patients not being of a habit fit for the administration of chloroform; and I have often heard of patients who, it was said, could not be brought under its influence; but I have never met with any of this sort. I failed several times to render patients insensible when I first commenced the use of it; but I am convinced that it was not from the patient's habit, but from my not being aware that it affected people in such a great variety of ways; and I am also convinced that, if other gentlemen fail in easily rendering their patients insensible, it is from the very same reason that perplexed me, or perhaps from employing an improper article, and not from the temperament of the individual.

It is well known to the Society, that in the practice of the dentists of Edinburgh, chloroform is habitually and generally employed. I

believe, however, that very few gentlemen of my profession in the larger towns in England use chloroform before extracting teeth, from the difficulty of obtaining a pure article. In Liverpool especially, I understand what is manufactured is so bad (causing nausea, tightness of breathing, headache, and other disagreeable symptoms), that the use of it is almost abandoned by the profession; and I am not surprised at such a result, for the greater quantity of chloroform manufactured there is made with naphtha, the smell of which distinctly remains in the preparation. I have sent up a good deal of what is made in this town to be tried there, and I am informed that it is a totally different article from what they have been attempting to use, and that the effect of ours is such as they were led to expect from the accounts published by Dr Simpson.

Now, I hold that chloroform is not accountable for patients being made sick and disgusted with all those mixtures that bear its name; and till gentlemen are careful, and do not use any except the pure article, we shall have many accounts of patients being riotous, sick, and ill, and, consequently, of medical gentlemen temporarily abandoning the use of it.

One very curious effect that I witnessed whilst administering chloroform to a patient whose intellect was very much impaired from some affection of the head, so as to leave her imbecile, was, that whilst under the influence of it, her eyes being open, her countenance lost the vacant expression habitual to it; and she looked round in my face with an intellectual expression, so calm and so rational that I was perfectly startled. Whilst gazing at her, however, the effects of the chloroform passed off, and she awoke with the same unmeaning laugh and vacant gaze that she had before. I mentioned this circumstance to my friend Dr Simpson the day that it occurred, and I thought of trying what effect the habitual use of the chloroform might have upon the patient by stimulating the brain so as to assist the cure; for this patient, I was informed by the parents, is gradually, though slowly, recovering the use of her faculties; and we do not know but that, by stimulating the brain, it might materially assist in the recovery; but I must leave this for others better acquainted with affections of this sort than I am, and recur to my own department.

As to the method I follow whilst my patients are recovering from the anæsthetic state, it is this—At first I allow the patient to remain quiet for half a minute or so, so as not to excite him—then the mouth being open (for it generally remains so after the gag is removed) I put a napkin quietly to it, to prevent the blood running down and destroying any part of the dress, and then merely fan the face briskly for a short time. The patient then opens his eyes, laughs, says, "Is it out?" or "That's very odd!" and the whole scene is finished by his getting up and saying, that he will never suffer toothache again when he can get quit of it so easily. The advantage of the use of chloroform to the operator is as great as to

the patient, as he can take more time to his operation than he formerly used to do when he knew that his patient was in an agony of anxiety, with all his courage screwed up to keeping his mouth open, his head steady, and his hands convulsively clutching "the horrid chair." Now, on the other hand, the scene is changed; and patients have even professed themselves to me to be glad to have toothache for the apology of getting a dose of chloroform.

In conclusion, I would beg to remark—

1st, That out of above 300 cases of dental surgery in which I have employed chloroform, I have never seen the least deleterious effect from its use, but the reverse.

2d, I have seen or traced no after bad consequences of any kind whatever.

3d, I have seen no case in which I have been afraid to give, and where I have not given it quite successfully.

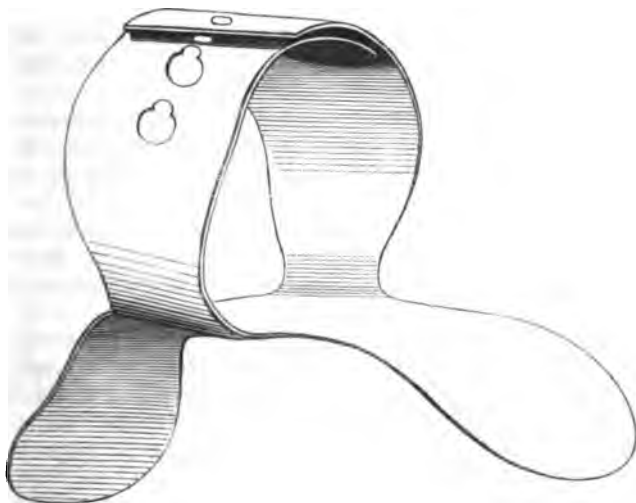
4th, It saves the patient's present physical suffering and previous struggles of feeling.

5th, It enables the dentist to perform his work with more satisfaction, certainty, and success.

6th, Patients, who have once had a tooth drawn under the influence of chloroform, invariably demand the repetition of the chloroform on requiring again the same operation.

ARTICLE V.—*Note on the Mouth Speculum.*

By ALEXANDER FLEMING, M.D.



I FIRST saw this useful instrument about two years ago in the wards of M. Rayer, at La Charité, Paris. It consists, as the annexed

wood-cut shows, of a circular band of metal (German silver) to keep the mouth open, while a broad horizontal plate, passing backwards, serves to depress the tongue. It facilitates greatly all operations on the mouth and throat, and is especially useful in children. By its aid, ulcers on the fauces may be cauterized much more accurately and more efficiently than without it. It is made of two sizes, for children and adults; and each speculum can be varied in size, to a certain extent, to adapt it to individual cases. Charrière of Paris has constructed a mouth speculum on the principle of his speculum vaginæ. It is considerably more expensive than the simpler one here shown; but has this advantage, that the same instrument can be enlarged or diminished in size to suit all ages. Specimens of both forms may be seen at Mr Young's, cutler, Edinburgh. As the mouth varies much in length in different persons, I have suggested to Mr Young, that in future he should make the tongue portion of two pieces, which, by a simple mechanism, may be so arranged as to admit of that portion being lengthened or shortened, as occasion may require.

Part Second.

REVIEWS.

On the Nature and Treatment of Stomach and Renal Diseases; being an Inquiry into the connexion of Diabetes, Calculus, and other Affections of the Kidney and Bladder, with Indigestion. By WILLIAM PROUT, M.D., F.R.S., Fellow of the Royal College of Physicians. Fifth Edition, revised. London: 1848. 8vo, pp. 595.

Urinary Deposits, their Diagnosis, Pathology, and Therapeutical Indications. By GOLDING BIRD, A.M., M.D., F.R.S., Fellow of the Royal College of Physicians, &c. Second Edition. London: 1846. 12mo, pp. 356.

Lectures on the Urine, and on the Pathology, Diagnosis, and Treatment of Urinary Diseases. By JOHN ALDRIDGE, M.D., Lecturer on Chemistry at the Medical School, Park Street. Dublin: 1846. 8vo.

Annuaire de Thérapeutique, de Matière Médicale, de Pharmacie et de Toxicologie, pour 1841, 1842, 1846 (Supplément), et 1848. (Mémoires sur le Diabète Sucré ou Glucosurie.) Par le Dr A. BOUCHARDAT, Agrégé de la Faculté de Médecine de Paris. Paris: 1848. 32mo. *Therapeutic Annual, &c.* By Dr A. BOUCHARDAT. (*Memoirs on Diabetes.*)

SOME of the works placed at the head of this article are probably so well known to our readers as to render a detailed criticism or

analysis a superfluous task on our part. This is particularly the case with the excellent work of Dr Prout, which has now attained such a European celebrity as to have scarcely any claim for notice from the reviewer; and the appearance of a fifth edition of which is a sufficient evidence of the estimation of the public. But the additions of late years to the sum of our knowledge of urinary diseases, are so interesting both in a scientific and practical point of view, that we have seized the present opportunity of bringing before the notice of our readers some of these, which scarcely appear to have received the consideration they deserve, either in Dr Prout's treatise or in any other systematic work that we have seen on the subject. We shall, therefore, occupy the greater part of this article with an analysis of such of these researches as appear most important; and to this end we shall dismiss, in as short a space as possible, the critical part of our office, by noticing in a few words the characteristics of the works before us.

In the present edition of Dr Prout's book, the body of the work presents little or no difference which can be justly called *original*, from the edition of 1843. The principal additions are in Chapter III. of Book First, where the views of Drs Johnson and Simon, on the pathology of certain renal diseases, are incorporated into the work, chiefly by means of large extracts from their papers, illustrated by some of the plates from the former author's memoir. A number of facts are also added to the account of the chemical constitution of the urine, selected chiefly from the memoirs of Golding Bird and Bence Jones, and from Liebig's late researches on the chemistry of food, which the author seems to value more highly than most of that chemist's productions. In the notes throughout the work there are numerous additions, the principal object of which is the support of the author's original views in points in which they appear to have been superseded or endangered by the researches of more modern chemists. Indeed, the effect of Dr Prout's acquaintance with such researches in modifying his own views, may be expressed in his words as applied to Liebig in the preface to the fourth edition:—"Some of the views" (of Liebig) "are the same as I have long advocated. Others are directly opposed to mine, and seem to me neither susceptible of proof, nor even probable."

From the whole tenor of the present edition, as well as the last, of Dr Prout's work, it is evident that the increasing cares of practice, and constant mental occupation arising from this cause, have driven him in a great measure from the field of experimental science, of which he was formerly so distinguished and successful a cultivator, into a somewhat too narrow and exclusive dogmatism. It is not possible for us to conceal from ourselves, that his theories have assumed in his mind that fixed position which is referred to by Bacon as one of the chief sources of the *Idola Tribus*, or *delusions of all humanity*; viz. where facts are assimilated to the theory, instead of the latter being modified by the facts; and where the mind, easily moved and

excited by affirmative instances, allows the negative to drop without sufficient investigation. But while our duty obliges us to ascribe to a truly great man some of these common errors of humanity, let us not omit to say, that those theories, as well as the most elaborate and praiseworthy experiments on which they were originally founded, have in many of their most cardinal points been corroborated by further experience, and remain not only among the noblest ornaments, but also among the most valuable lights of the present age. While we cannot accept Dr Prout's work as the expression of the *whole* of modern science on the subjects of which he treats, we are still of opinion that the student will find in it not only a collection of most instructive practical facts, but a mass of sound and irrefragable principles, in relation to the processes of secretion and assimilation, such as, amid the fluctuating opinions of modern writers, he will not easily acquire from any other source.

We have on a former occasion referred to Dr Golding Bird's treatise in terms of the highest praise; and constant reference to it since that period enables us to renew our testimony to its general accuracy, and to recommend it as by far the best guide to the examination of urinary deposits.

The work of Dr Aldridge has been for some time before the public, and we regret that our intention of noticing it has not been sooner carried into execution. It consists of a series of six lectures, giving a succinct and clear summary of the most important facts in reference to the chemistry of the urine, and the pathology and treatment of urinary diseases. The work is also characterised by many very original and striking views, some of which will be noticed hereafter. It is exceedingly creditable to the author, that while chemical details have occupied so much of his attention, he has not allowed the pathological anatomy of the urinary organs to fall into that secondary place which it occupies in the works of many chemical writers. In this respect the lectures of Dr Aldridge are diametrically opposed to the work of Prout. The former author is more of what we might call a *solidist*, the latter is essentially a *humoralist*. The former wishes in every possible case "to connect the functional disorders with the organic lesions on which they depend," or at least always "to define the groups of symptoms to which may be philosophically given the name of a distinct disease." He accordingly tends to look upon all urinary derangements as the result of some definitive lesion of the glandular or excretory apparatus, or some recognized morbid constitution of the system, such as the rheumatic, gouty, scrofulous, &c., acting upon the urinary organs. On the other hand, he rejects in great part those functional disorders of assimilation, which make up nearly the whole sum of Dr Prout's philosophy, and of which, according to him, the alterations in the urine are most frequently only the secondary results. Thus he considers the terms *phosphatic* and *lithic diathesis* as leading to erroneous and unphilosophical views of disease; and represents strongly, by way of

analogy, "the obscurity and confusion which would result from sitting down to prescribe for a rusty sputa diathesis, or a mucous expectoration diathesis," in a case of pulmonary disease.

Although we have already expressed our high admiration of the spirit which has actuated Dr Aldridge in setting pathological anatomy before him as a guide in the investigation of urinary diseases, we must object at once to the above analogy, and to the doctrine which suggested it; to the analogy, because it depends on an imperfect apprehension of the word *diathesis*; to the doctrine, because it cuts the gordian knot which binds together all facts having reference to the functions of assimilation and secretion, which knot it was the endeavour of Dr Prout's book honestly to untie. While we admit that Dr Prout looked too exclusively to the aliment and the blood for a solution of the phenomena of urinary diseases, and paid too little attention to the vitiated condition of the secreting organs themselves, we contend that, on the other hand, an equally exclusive spirit is manifested, and equal or more glaring errors are unavoidable, when it is attempted to confine the sources of urinary derangement within the comparatively narrow range of pathological alterations, to which the urinary organs themselves are subject. That this has been a frequent source of error in Dr Aldridge's work we shall be prepared to maintain, when we examine his views in detail; meanwhile, as it is important that our meaning should be understood *in limine*, let us analyze for a moment the illustration drawn from pulmonary diseases.

For the due performance of the chemical function of respiration, it is sufficient that atmospheric air be brought in contact with the pulmonary mucous membrane. The conditions, therefore, of the efficiency of this function are the integrity—1st, Of the pulmonary circulation; 2d, Of the air passages; 3d, Of the mechanism by which respiration is carried on. These conditions entitle us to look for the source of the greater portion of respiratory derangements in obstructions of the air-passages either by foreign bodies from without, or by exudations from the mucous membrane itself; and of these obstructive exudations "mucous expectoration" is known by experience to indicate disease in one locality, and "rusty sputa" disease in another locality. Hence experience teaches us to consider rusty sputa and other abnormal expectorations not as the evidence of a *diathesis*, but as being the concomitants of grave symptoms of respiratory embarrassment, and, along with these, the proof of lesion of the respiratory organs.

With the urinary function the case is different. The formation of a perfectly normal urine implies a wide series of conditions, among which the most essential are—1st, A right digestion and formation of chyle; 2d, A formation of normal blood; 3d, A right secretion by the skin, liver, and all other secreting organs; 4th, Complete and perfect respiration; 5th, A due degree of waste of all the tissues, and a correct proportion of this to the amount of supply.

Not only, therefore, is it evident that many temporary derangements of the urinary secretion must exist which cannot be considered with propriety as the evidence of disease at all; but also that habitual alterations of that fluid may and must be produced wherever there is habitual mal-assimilation, or indeed continued derangement of any of the important functions above mentioned. Such derangements of the urinary secretion are the *physiological compensation* for errors of diet, or disease of other organs, and cannot, with any propriety, be introduced into the list of urinary diseases.

To take an example. If, from suppressed cutaneous secretion, excessive animal food, rheumatism, or any other cause, an individual becomes affected with habitual secretion by the kidneys of an excess of lithate of ammonia, which thus physiologically compensates for the imperfection of other functions, this derangement, though habitual, is not a urinary disease; it is merely a diathesis, indicative of disease or error in some more or less remote function. This diathesis might indeed become the cause of disease in the urinary organs, should it lead to the formation of a calculus in the kidney, and give rise to irritation there; but in its simple form it is a reparative not a destructive agency, and, in relation to the kidneys, denotes a physiological, not a morbid condition.

We are by no means disposed to regard the alterations of the urinary function as being less dependent on the integrity of the organs allotted to it than those of any other function of the human body. But the renal diseases, properly so called, have yet to be studied in relation to a new pathological anatomy; and the followers of Bright and Rayer have not yet brought their inquiries to any very satisfactory conclusion. In the mean time, the great idea of the mutual dependence of the processes of secretion and assimilation, the origination of which by Prout formed one of the greatest and most practical discoveries of our age, has been worked out in a variety of forms, and to some of these we propose in the present review directing the attention of our readers. We here bring to a close, therefore, these somewhat lengthened preliminary remarks, and approach the subject of

DIABETES MELLITUS.

Semeiology and Diagnosis.—The general symptoms and distinguishing characteristics of diabetes have been very completely described by Prout, Bardsley, and Christison; and comparatively little has been done by more recent writers on this branch of the subject. There is, indeed, little difficulty in distinguishing this disease; the great thirst, the inordinate appetite, the suppressed cutaneous exhalation, the decline of strength and flesh, and the excessive quantity of the urine, scarcely ever failing to excite attention, while its saccharine character, and increased density, may be considered as completing the diagnosis. It is important to know, that according to Dr Golding Bird (*Lond. Med. Gazette*, 1844), the presence of

sugar in the urine can frequently be recognised long before any increase of density has taken place, and before any characteristic symptom has occurred; a circumstance which has also been observed by Dr Prout in dyspeptic individuals.

The very simple tests which have lately been placed in the hands of the practitioner, for the detection of small quantities of sugar in urine, facilitate very much the clinical study of diabetes, especially in its early stages. We believe, that of all the tests which have been proposed for this purpose (an account of the best of which will be found in Dr Bird's work, p. 291), that proposed by Mr Moore is at once the most convenient and accurate. It consists in boiling the suspected urine with half its volume of *aqua potassæ*, when a brown colour, more or less deep, is readily developed in a fluid containing even a very small quantity of grape or diabetic sugar.* This test is better adapted for general use than those of Trommer or Capezzuoli, which depend on the de-oxidation of the hydrated oxide of copper; as these tests, though very minute, and in general accurate, are subject to the objection, that occasionally a somewhat similar reaction is produced by apparently normal urine.—(See *Monthly Journal*, June 1844, p. 564.) The test proposed by M. Biot, depending on the power of saccharine solutions of producing a deviation in the rays of polarized light, is stated by M. Bouchardat to be very accurate, and to be capable, by means of a formula which he has given, of indicating the amount as well as the existence of sugar. We believe this test to be worthy of more attention than it has received in this country. For practical purposes, however, the quantity of sugar may be sufficiently easily estimated by the well-known method of Dr Christison, which consists in confining a portion of urine in a graduated tube, over mercury, and causing it to ferment by the addition of yeast; when every 47 cubic inches of carbonic acid gas discharged will be found to correspond to 45 grs. of sugar.

Pathology.—On the pathology of diabetes we have to record numerous new facts and opinions of the greatest importance. Beginning with those views which appear to have a sufficient demonstrative or experimental basis, we shall find that diabetes has been, by the progress of science, completely removed out of the pale of mere primary disease; and that the light shed upon the primary assimilative processes of late years by the inquiries of Blondlot, Bouchardat and Sandras, Magendie, Bernard and Barreswil, have, more than any other circumstances, tended to elucidate this obscure affection. We are aware that the idea of diabetes as an assimilative disease is by no means new, and may indeed be said to have guided English pathology since the time of Rollo; but the complete proof and satisfactory

* M. Bouchardat employs milk of lime, which darkens the saccharine urine in the same way as potass, and is sometimes more readily accessible.

application of this idea are of much more recent origin. We shall endeavour to give our readers a clear view of the elements on which the case rests.

The first decided advance in the direction we have indicated was undoubtedly caused by the publication of the papers of Mr Macgregor of Glasgow (*Med. Gazette*, May 1837). By showing that the abnormal saccharine ingredient is not confined to the urine, but exists also in the saliva and fæces of diabetic patients, the supposed connexion of the disease with a special renal disorder was rendered very improbable; and by the confirmation of the experiments of Ambrosiani and Maitland, as to the existence of sugar in considerable quantity in the blood of diabetics, the morbid element was traced still nearer its source. The experiments of Macgregor, however, were especially valuable in relation to the examination of vomited matters in the healthy and diabetic states, and lead directly to the conclusion, not only that the sugar of diabetes is found in great quantity in the stomach, but that even in healthy assimilation a small quantity of the same substance is produced.¹

In addition to the experiments on sugar, Mr Macgregor adduced evidence to show that the quantity of urea (which had been originally supposed by Prout to be the source of the sugar in diabetic urine), is not in reality diminished, and that the sugar is, therefore, in all probability an *added* constituent, instead of a product of decomposition of any of the elements normally present.

From these researches, therefore, it appears—1st, That the production of sugar in the stomach during digestion to a small amount is a natural process; 2d, That this ingredient is formed by diabetics in the stomach to an abnormal extent; 3d, That the existence of sugar in the blood to more than a very small extent, is peculiar to the diabetic state; 4th, That in diabetics the abnormal sugar appears in the form of an added constituent in the fæces, saliva, and urine, being probably carried out of the system chiefly by the latter secretion. The increased flow of urine would thus seem to be a secondary result, the consequence of the presence of large quantities of an abnormal ingredient in the blood.

In the last two editions of his work Dr Prout has stated very clearly the theoretical views which appear to spring from the researches above mentioned, although he appears to be ignorant of the further experimental confirmations which these have received, and which we shall presently relate. As the quantity of sugar which exists in healthy blood is insignificant, and as sugar is frequently ingested in considerable quantity with the food, it is clear that there must exist in the organism a *faculty of converting the saccharine*

¹ It appears, by the recent researches of Magendie, that in animals fed on feculents a small quantity of diabetic sugar or *glucose* is found normally in the blood, though not enough to affect the secretions.—(See *Comptes Rendus*, 1846.)

principle into some of the elements of the blood. This faculty or function must be of great importance, and its derangement or destruction is the real source of diabetes, which is therefore a derangement of the secondary rather than the primary assimilating processes.

This conclusion is, we think, fundamentally the same as we shall presently show to have resulted from more enlarged views of the assimilative processes, and more precise experiments.

One of the most important results of recent investigations into the phenomena of digestion, is the establishment of the important fact, that the secretions poured into certain parts of the alimentary canal have the power of converting fecula or starch into *glucose*, a form of the saccharine principle identical with diabetic sugar. For the full development of the details on which this fact rests, we are indebted to various inquirers, each of whom individually appears to have taken too exclusive a view. The saliva was indicated several years ago, by Leuchs and Burdach, as the chief agent in the solution of the feculents, which, they maintained, were converted by it into sugar. These views were more recently taken up by M. Mialhe, who, in March 1845, read to the Academy of Sciences in Paris a memoir, in which it was satisfactorily shown that the saliva, as taken from the mouth, contains a principle very similar in its properties to *vegetable diastase*, and possessing in a high degree the power of that agent in converting starch into saccharine matter or glucose. Having ascertained this fact, M. Mialhe sprang with characteristic eagerness to the conclusion, that the saliva is the sole fluid which has the power of digesting the feculents,—a conclusion which was shown to be incorrect by M. Bouchardat, who had previously (in January 1845) read a memoir to the Academy, proving, by experiments on animals, that it is chiefly in the duodenum and small intestine that this change is effected. M. Bouchardat admits the accuracy of M. Mialhe's observations with respect to the saliva; but he has succeeded also in obtaining a *diastase* from the pancreatic fluid, and in proving that the fluid obtained by digesting the fresh pancreas of an animal in water has a strong reaction upon fluid starch. His experiments further show, that in animals which feed upon crude fecula, that substance is little affected in the mouth or even in the stomach, the saliva not having the power of rupturing at once the external membrane of the starch vesicles, which therefore remain intact till after they have remained a considerable time in contact with the fluids of the alimentary canal. M. Bouchardat further established, negatively, that neither the bile, nor the gastric fluid in its normal state of acidity, have the power of acting on the feculents; and he concludes, therefore, that in health the principal digestion of these substances takes place in the duodenum and small intestine, under the influence of the pancreatic fluid; and that it is only where the fecula forming the food of the animal has been previously cooked, that the fluids of the mouth have the power of acting on it.

These conclusions of M. Bouchardat are supported by a series of

experiments which we cannot give at length, but which appear to us to be quite conclusive as to their general accuracy.—(See *Annuaire de Thérapeutique*, Supplément 1846; *Mémoires sur la Digestion des Féculents et sur les Fonctions du Pancréas*.) Nevertheless the subsequent experiments of MM. Bernard and Barreswil deserve notice. They find that the power above ascribed to the saliva and pancreatic fluid belongs also in a marked degree to the gastric secretion, *provided that the acid naturally existing in that secretion be neutralized by an alkaline carbonate*; and conversely that the saliva and pancreatic fluid, *on being treated with an excess of acid*, acquire the power, formerly supposed peculiar to the gastric secretion, of digesting and dissolving the albuminous or protein compounds. It would thus appear that, with respect to the feculents and the protein compounds, the three fluids above mentioned exert a common digestive power, which is modified in each by the circumstance of their acid or alkaline reaction.¹

M. Magendie carries this generalization still further. In a memoir read to the Academy of Sciences (*Comptes Rendus*, 1846), he states that most of the composite secretions of the animal economy possess the power of converting starch into glucose. It is still more remarkable that the blood appears to possess this power in a very considerable degree; for, on injecting a solution of starch into the veins of an animal, the blood soon after gave evident traces of glucose.

To sum up the above physiological digression, it appears that the digestion of feculents into glucose takes place normally in the upper parts of the alimentary canal wherever the reaction is alkaline; while the solution of protein compounds occurs in the presence of an acid reaction. It follows that the latter process takes place in the normal state in the stomach; while the former, begun to a very slight extent in the mouth, is mainly carried on in the duodenum, under the influence of the pancreatic secretion. This conclusion corresponds perfectly with the results of Bouchardat's experiments on animals above alluded to.

We have been thus particular in explaining the facts in regard to the digestion of feculents in the normal state, because a clear view of this subject is, we conceive, quite essential to the right understanding of the pathology of diabetes. According to M. Bouchardat, (who has made by far the most extensive and satisfactory series of observations on this disease of late years) diabetes is invariably dependent on an abnormal digestion of the feculents *within the stomach*, instead of lower down in the canal. This fundamental fact he has, we think, proved, not only by numerous observations on the contents of the stomach during digestion in diabetes as compared with

¹ *Comptes Rendus*, vol. xiv. M. Bernard has lately ascribed to the pancreatic fluid the power of dissolving the fatty acids. (See *Retrospect* for July, No. 244); but this has no relation with the present subject.

healthy individuals (which observations agree in the main with those of Macgregor before noticed) ; but by showing that the fluid from the empty stomach of diabetics has, *even when acid*, a power not possessed by the normal fluid, of converting starch into glucose.—(*Annuaire*, 1848, p. 229). M. Bouchardat considers this peculiarity due to the presence of a diastase, which is absent in the healthy stomach, and which is secreted there in virtue of a vicarious action, the pancreas being, in his opinion, incapable of performing its office in diabetes. This last point, however, is by no means well made out, depending chiefly on a single observation of diseased pancreas in connexion with diabetes.

The effect of the formation of glucose in the stomach is, according to M. Bouchardat, its immediate escape into the blood by the veins in quantities too great for secondary assimilation. Thus arises the necessity for its being carried off by the kidneys. Of this the diuresis is a secondary effect ; while the intense thirst is in consequence of the large amount of water required to convert the feculents swallowed into glucose. The habitual low temperature of diabetic patients is explained by the large quantity of cold water drunk, and the loss of caloric which occurs from the conversion of large quantities of starch into sugar.

The most defective part of M. Bouchardat's theory is his explanation of the phenomenon, that sugar when formed in the stomach has more tendency to pass into the blood unchanged than when formed in the duodenum and jejunum. He ascribes this difference to the influence of the portal circulation, which he considers as exerted solely on matters absorbed from the duodenum, forgetful that the venous system of the stomach, like that of the rest of the alimentary canal, is connected with the circulation in general only by means of the vena portæ. This singular oversight makes it necessary to look at this part of the subject with other eyes than those of M. Bouchardat.

The secondary assimilation of saccharine substances, or their conversion into the normal elements of the blood, has undoubtedly much to do, as Prout has supposed, with the pathology of diabetes. But the whole of this subject is still enveloped in considerable obscurity. M. Bouchardat's own experiments show (*Sur la Digestion des Sucres, Supplément de l'Annuaire*, 1846), that when given in very large excess to animals, sugar is in part decomposed in the stomach into lactic acid, but is found in considerable quantities in the blood and bile, and in smaller (sometimes doubtful) in the chyle of the thoracic duct, and in the urine. From its large quantity in the bile, as well as from some other considerations, on which we shall not dwell at present, it is not improbable that, as M. Bouchardat supposes, the liver is endowed with a peculiar power of separating the superfluous or noxious substances which enter the circulating system. That it has the power of converting glucose into the elements of the blood, however, we see no reason to believe, and are

therefore at a loss to know how sugar should be transformed in the portal any more than in the general circulation. We incline to ascribe the conversion or secondary assimilation of sugar to the mesenteric glands, or some part of the lacteal system. It is clear that the lacteals have the power of selecting from the contents of the intestine certain nutritive elements, and, on the other hand, of rejecting substances unfit for assimilation; thus, in M. Bouchardat's experiments (*Annuaire* 1846, *Suppl.* p. 89), saffron and prussiate of potash having been introduced with the food, the chyle was found to contain neither of these substances, while the blood contained both. Experiments are still wanted to determine the composition of the chyle in the lacteals as compared with that in the thoracic duct; but in the mean time it appears to us very probable, that the lacteals, along with oleaginous and albuminous matters, absorb glucose from the intestines; and that this latter substance is decomposed in the mesenteric glands into other elements, thus accounting for its absence, or presence in very small quantity, in the thoracic duct. It is worthy of remark, that alkaline fluids have, even out of the body, a power of acting slowly upon glucose, which gradually disappears when dissolved in water along with alkaline carbonates. —(*Op. cit.* p. 100.)

If this speculation be correct, then the real difference between diabetic and normal assimilation is probably the following: In the former, the glucose produced in the stomach is subject, in the first instance, to venous absorption *only*, and in a situation where we know that venous absorption very readily takes place; and accordingly the blood becomes loaded with unassimilated saccharine matter. In the latter, on the contrary, the glucose produced in the duodenum is at once rapidly absorbed by the lacteals, and none, or at least a very small quantity, passes unchanged into the circulation. This small quantity, if present, is probably either eliminated by the liver in the healthy individual, or undergoes a slow destruction in the blood, which, from the free alkali present, possesses the same power as the alkaline solutions above adverted to.

Such is, we believe, a faithful summary of what is really known, or plausibly supposed, upon the subject of the pathology of diabetes. Did our space permit, we should advert to the ingenious, though we believe erroneous, theories of other authors; as it is, we can only advert in a few words to the views of Mialhe and Dr Aldridge.

M. Mialhe, the opponent on some points of Bouchardat, maintains that in diabetes the natural alkaline reaction of most of the animal fluids is either destroyed or very much impaired. He asserts that he has found the blood of diabetes neutral, or verging on an acid reaction; Bouchardat again denies this, alleging that the blood of diabetics is, like other blood, alkaline when drawn from the vein, but that it has a tendency to become acid when kept, from the presence in it of glucose, which is converted into lactic acid. In other respects the views of M. Mialhe are mostly hypothetical.

Dr Aldridge considers the sugar in diabetes to be formed in the urine, together with ammonia, by the decomposition of the albumen of the blood. "Albumen is formed in vegetables," he says, "by sugar combining with ammonia, and the mixture losing a certain quantity of water and oxygen. If albumen is formed by the union of sugar and ammonia, it is plain that the two latter compounds may be formed by the decomposition of albumen." However ingenious and plausible this chemical theory may be, it is sufficiently answered by the fact, that the sugar of diabetes is *in the blood ready formed*; a fact which Dr Aldridge should have known. This theory of diabetes is eminently subject to the objection which we have made against the general principles of the work which contains it; that of attempting to localize in the kidneys diseases which have their primary seat in other organs.

Treatment.—The treatment of diabetes by specific remedies, and with a view to its entire cure, is a subject which is exceedingly apt to be used by the enemies of medicine as one of its *opprobria*. In this, as in other departments of our art, it is most true, that extraordinary and unfounded theories have always produced a miserable fruit of useless, or worse than useless, remedies; so that, in the words of Dr Prout, "perhaps there is no disease in which so much mischief has been done on false principles, and by random experiment," as in diabetes. Still it is undeniable that there are few diseases in which so much good may be temporarily effected by proper hygienic and dietetic regulations, aided by a skilful and not too zealous employment of medicines. Of the number of diabetics who enter our hospitals, few perhaps leave them without a decided amelioration in the more obvious symptoms. A few, perhaps, are even apparently cured; but their return at no distant period is still counted on by the enlightened physician with the same degree of probability as in a case of phthisis or Bright's disease.

The conclusion, therefore, has been growing in strength, and is now almost inevitable, that while the ultimate cure of diabetes is rarely accomplished, and is not to be compassed *directly* by the resources of art, the circumstances which destroy the comfort, and shorten the life of the unfortunate patient, are capable of being to a great extent modified or controlled. The treatment of diabetes in this sense, we hold to be one of the triumphs of enlightened pathology and scientific medicine; for in this case, as in all others where great results have been attained, science and practice have exercised a mutual influence; and while experience has corrected unsound theory, sound theory has held the torch to experience, and led the way to judicious and successful trials.

It will reasonably be expected from these remarks, that the treatment of diabetes shall have been found to correspond, in some degree, with the improved views of its pathology, which we have endeavoured to enunciate. So much has this been the case, that, even at the risk of being considered too exclusive, we shall set aside in a great

measure the host of empirical or questionable remedies which have been proposed, and proceed at once to the detail of the treatment which we conceive to have the firmest basis both of theory and experience.

The main feature of this treatment has been recognized since the time of Rollo; and is simply the exclusion from the aliment of all substances capable of producing sugar or glucose, which in diabetes is known to be imperfectly assimilated. It is in the application of this method, however, that recent writers, especially Bouchardat and Prout, have chiefly modified the method laid down by Rollo. The exclusively animal diet which, up to a recent period, was generally considered all-important in the treatment of diabetes, was so insupportable to the majority of patients, that it was with extreme difficulty, both in hospital and private practice, that a sufficient strictness of regimen was obtained. Nevertheless, the results obtained by those who subjected this method to the severe trial which was necessary, showed that a power was in this way obtained over the disease which had been till then quite unknown. But independently of the difficulty of carrying it out at all, the animal diet was found to have its disadvantages. It was found that diabetics, instead of having, as was supposed, stomachs all-powerful for the digestion of animal food, were readily injured by indigestible substances; that cheese, butter, fat, therefore, which had been contemplated not only as additions to the diet, but as forming important and even essential parts of it, had often to be discarded, from their not agreeing with the individual idiosyncrasy, which it was discovered required quite as much attention in diabetics as in any other invalids. Hence it was found, that though, when an animal regimen could be maintained with an ordinary degree of care, the morbid symptoms referrible to the urine were readily kept under command, the advantage to the general health was not always commensurate with the apparent success of the treatment; and physicians, as well as patients, were constantly induced to break through the iron rule of Dr Rollo, from the conviction that an exclusively animal diet was apt to be hurtful in its own fashion, and to produce in the patient a disgust for food, which could not but be very injurious.

From the difficulty, and even danger, in some cases, of following out the exclusively animal diet, various modifications of it have been allowed by different practitioners; most of these, however, cannot be considered as any more than a compromise of expediency between the desires or even necessities of the patient, and the rigid system before referred to. It is to M. Bouchardat, so far as we are aware, that the merit belongs of first enunciating and carrying out the principle, *that the dietetic treatment of diabetes depends not on the suppression of vegetables in general, but of saccharine and farinaceous matters in particular.* This principle, which is perfectly in harmony with the theory above detailed, has been submitted by M. Bouchardat to an extensive trial. Forty-five cases of the disease have now

been recorded by him, observed with great accuracy and precision, and several of which have been under his observation for many years. The results are, 1st, that where the saccharine and feculent elements can be completely excluded from the food, the abnormal quantity and character of the urine can *always* be completely controlled; 2d, that by persistence in this system, and attention to other hygienic regulations which we shall presently mention, the health of the diabetic patient may often be re-established, and a *virtual* cure effected; 3d, that in this state the morbid habit is often so far subdued, that a very moderate amount of feculents may be used for a time without any bad consequences, provided a constant attention be given to the state of the urine, and the regimen be resumed upon the least threatening of a relapse. It does not appear certain, from an examination of M. Bouchardat's cases, that a cure, *in the strict sense of the term*, has been obtained in any instance. In some cases, indeed, the tendency to relapse, even under a regimen by no means absolute, appears not to have been observed for a very considerable period; and there are good grounds for hoping, that in such individuals the state of apparent health may be prolonged indefinitely; but strict attention to hygienic regulations, and extreme moderation in the use of feculents, appear to have been necessary, in all instances, to this immunity; and several cases of relapse after considerable intervals of health, prove that the security of such persons is not to be considered as perfect.

Let us not, however, in stating the fundamental principle of M. Bouchardat's treatment, fall into an error, which he himself repeatedly warns us against; that of looking to the restriction of the diet alone as the means of cure of the diabetic condition. Admitting that the urine may be thus rendered normal, it must be recollected that there is a perverted cutaneous function to restore, a bodily and mental debility to remove, a tendency to secondary disease to be assiduously guarded against. To fulfil these indications, it is imperatively necessary, 1st, that the patient be clothed in warm flannels, worn next the skin, and over the whole body; 2d, that he take exercise in moderation; 3d, that he use baths; 4th, that his aliment be (within the prescribed limits) generous and varied; 5th, that a certain quantity of wine, especially claret (which has been found superior to all the other wines in France), be allowed; 6th, that these hygienic circumstances be assisted, if necessary, by tonics, stimulants, and diaphoretics, especially opium.

¹ Two cases have been recently published by Dr Todd (*Provincial Journal*, June 28, 1848), which, while they show in the most marked manner the favourable influence of a diet restricted with respect to feculents, appear in some respects to be opposed to the results obtained by Bouchardat. We think, however, that a minute analysis of these cases would prove their apparent results not altogether unexceptionable.

The only medicine to which M. Bouchardat appears to ascribe any thing like a specific power, is the carbonate of ammonia, given in doses of from one gramme (15 grains) to five or even ten grammes daily. In this drug he has a considerable degree of confidence as an adjuvant remedy; and he prefers it most decidedly to the bicarbonate of soda, so much vaunted in diabetes by M. Mialhe, whose theory we have already considered. Whether these remedies be justly praised or not we have no means of determining, but we can readily believe, with M. Bouchardat, that the treatment of diabetes by therapeutic means alone involves a most dangerous omission; and we suspect that the theoretical treatment of M. Mialhe, unless supported by a much larger amount of experience than he appears to have had, will not obtain many supporters.

We cannot avoid noticing here, that the large experience of Dr Prout appears to have been conducting him insensibly into the same path with M. Bouchardat. "It will readily be understood," says Dr Prout (p. 39), "that an animal diet ought to form an essential part in the treatment. I do not, however, approve of a diet exclusively animal; but consider a certain proportion of vegetable matters proper. The vegetable matters to which I particularly allude, are the green parts of plants. With regard to farinaceous matters, I have lately prohibited them altogether in the confirmed stages of diabetes." The "farinaceous matters" which in the *fourth edition* were recommended in this place as the most eligible form of vegetable food (see p. 40), are here prohibited; and in their place are substituted green vegetables, which form a most prominent part of the "aliments qui doivent être permis" of M. Bouchardat. Indeed the dietetic treatment of diabetes by Dr Prout, laid down in the present edition of his work as the result of his latest experience, appears to correspond in all essential particulars with the principles sketched above. It may be in part the result of this new system, that whereas in 1843 he had seen "*a few*" only, he has now (1848) seen "*many* cases in which the saccharine quality of the urine has for the time entirely disappeared; and a very great number of cases in which the symptoms have been so far subdued as to give little trouble to the patient" (p. 57).

As regards the details of treatment necessary or advisable, according to M. Bouchardat's method, we must refer to the Supplement of the *Annuaire* for 1846; we shall merely add a few words principally on the dietetic and most important portion. The list of proscribed articles is as follows: "Bread, whether of wheat, rye, or barley, &c. (or oats); pastry; rice, maize, and other grains; radishes; potatoes, and their starch; arrow-root, and other forms of starch; vermicelli, semolina, macaroni, and all similar farinaceous pastes; the leguminous seeds, haricots, peas, lentils, beans; chestnuts; buckwheat; all confections, and other sugared meats and drinks."

Almost all other articles of food are allowed, with due regard to the digestive powers of the individual. Variety is encouraged, and

every description of cookery allowed; so that even a diabetic epicure need not fare very ill. The longing after feculents, and particularly after bread, however, is generally very strongly felt; and, in order to gratify this taste, M. Bouchardat employs a bread made exclusively of gluten of wheat, from which the starch has been carefully removed by a stream of water. (The method of preparing gluten bread is described in a separate memoir, *Supplément pour* 1846, p. 311.) This bread, which is now regularly manufactured in Paris for the use of diabetics and dyspeptics, is quite digestible, and soon comes to replace the more ordinary article. It is not, as many have supposed, an essential part of the treatment of diabetes in M. Bouchardat's hands; but merely a substitute for ordinary bread, which may be employed without injury, if the desire of the patient for it is strong. Gluten may likewise be made into various dishes.¹

Fermented liquors are not allowed, on account of the dextrose they contain; but considerable importance is attached to alcoholic stimulants, such as wine, rum, and brandy, taken in moderation. Every excess, however, is to be avoided. Coffee is generally allowed, but must be taken without sugar, or, at least, with very little. Acid liquors, such as lemonade, are prejudicial; but Seltzer water, or such waters as Spa and Vichy, may be taken with advantage. The great thirst, however, complained of by diabetics, is found to be uniformly in proportion to the quantity of feculents consumed; and the regulation of the diet is usually sufficient to control it.

Of all the other precautions to be used in diabetes, the greatest importance is ascribed by M. Bouchardat to the diaphoretic regimen. The use of flannel, indeed, is with him an imperative regulation, subject to no exception. Not only is it useful in restoring the cutaneous transpiration, but in guarding against the supervention of the pulmonary disorder, to which all diabetics are constitutionally liable. Its effects are to be aided by baths, exercise, and the occasional use of opium in the form of Dover's powder, or of the antiquated theriac, the stimulant and diaphoretic properties of which make it a useful preparation, in spite of its strange composition.

We cannot in this review enter into an individual consideration of the numberless remedies which have been employed in diabetes, either purely empirically, or from some species of guiding principle. Astringents, and particularly alum, were among the oldest remedies employed in this disease; and have lately been recommended both by continental and British writers, particularly by Dr Aldridge. M. Bouchardat, while he does not deny their efficacy altogether, appears to have seen many cases in which no good effects followed; and in this respect we think he expresses the common opinion.

¹ Dr Prout recommends, as a substitute for bread in diabetes, a compound of bran, eggs, and milk, which is made into cakes, and baked in a quick oven. For the process, see p. 44. Fifth Edition.

Notwithstanding the strong recommendations of Watt, blood-letting has never been employed indiscriminately by judicious practitioners in general. By some it has been used to a considerable extent, when the pulse and increased heat indicated a degree of febrile reaction in the early stages of the malady. The temperature of diabetics, however, is more commonly below than above the average; and, considering the tendency which uniformly exists in this disease to death by sudden or gradual sinking, as well as to all the diseases of debility, we cannot but assent to the opinion of M. Bouchardat, that general blood-letting ought to be most sparingly employed, and never as a part of the ordinary treatment. Local blood-letting, in the form of leeches to the anus, he has sometimes employed, when the disease has succeeded to the suppression of a hemorrhoidal discharge; but his protest is most strong against excessive depletion in every form, unless employed with a special object.

We cannot conclude without urging upon practitioners in this country a fair and just trial of M. Bouchardat's method in diabetes. We consider that method to be founded upon just and legitimate inductions; and we believe the cases by which it is supported to be the fruit of sound and matured observation, and very different from the flimsy evidence upon which new therapeutic methods not uncommonly rest. On this point our convictions are strengthened by the consideration, that this method involves no radical or sweeping change upon those whose long-tried efficacy is all but universally admitted; but is more of the nature of a selection from those methods, based upon sound data both of science and experience.

The length of the present review precludes our entering, at present, further into a consideration of the important subjects we had in view in commencing. In a future Number, however, we shall probably recur to some of these.

W. T. G.

A Practical Treatise on the Diseases peculiar to Women; Illustrated by Cases derived from Hospital and Private Practice. Third Edition.

By SAMUEL ASHWELL, M.D., M.R.C.P.L. Late Obstetric Physician and Lecturer at Guy's Hospital. London: 1848.

IN noticing this third edition of Dr Ashwell's treatise, we cannot but express, in general terms, a very high opinion of its value as a practical work. Coming from a man of reputation, and based upon a vast and varied experience in the diseases treated of, it demands the serious attention and study of all inquirers in the field of the diseases of females. At the same time, we feel bound to state, that the book comes very far short of our expectations, inasmuch as it has no pretensions to keeping pace with the progress of science, or maintaining its position in regard to similar works, as for instance Dr Churchill's excellent treatise. On various subjects—particularly in

diagnosis and treatment—which we could cite, the well-confirmed opinions of the profession are set at nought in an unseemly manner, or no mention of them is made.

The easy and natural arrangement of this class of diseases into the two orders of functional and organic, is that adopted by our author. In discoursing on the functional disorders, he has, however, carried out this separation and distinction to a length that is not justified by a sound pathology. Still, his chapters on chlorosis, amenorrhœa, vicarious menstruation, dysmenorrhœa, menorrhagia, leucorrhœa, hysteria, irritable uterus, &c., are very valuable in a practical point of view.

Of the second part of this work, upon the organic diseases of the genital system, much cannot be said in the way of commendation. His chapters on organic diseases of the os and cervix uteri, and on the diseases of the internal genital organs, will be consulted by all with great advantage. We are sorry our space prevents us entering into details, and noticing some of the errors into which Dr Ashwell has fallen in this part of his work; and we can merely state our conviction, that on further study our author will find many changes demanded in his chapters on tumours of the uterus, on the treatment of pregnancy complicated with organic disease, on displacements of the uterus, &c. &c., changes which we shall look for in his next edition.

On the Cure of Cataract, with a Practical Summary of the best Modes of Operating (Continental and British). By HUGH NEILL, Surgeon to the Liverpool Eye and Ear Infirmary. Liverpool: 1848. 8vo. Pp. 224.

THE author has undertaken to present the reader with an “analytical review of the leading contributions of eminent continentalists on the cure of Cataract” (Preface, p. 2). Among others, the treatise of Desmarres,¹ whose clinique on diseases of the eye is at present the most instructive in Paris, receives a lengthened analysis. We have been familiar with this work since its appearance in 1847, and fully agree with Mr Neill in the high opinion he entertains of it. We regret to find, however, that Desmarres’s opinions are most imperfectly rendered in the present work. Indeed, his meaning is often completely misunderstood by Mr Neill, who has so mixed up his own opinions and experience with those of Desmarres, that it is impossible for the reader to say what belongs to the one, and what to the other, unless, for his guidance, he adopt the plan—which he may with considerable safety—of referring all the sense to Desmarres, and all the nonsense to his reviewer.

¹ *Traité Théorique et Pratique des Maladies des Yeux*, par L. A. Desmarres, M.D. Paris: 1847. Pp. 904.

After a very unfair statement of the relative merits of extraction and reclination of the lens in cataract, he expresses a decided preference for the latter. In gaining experience for the operation of extraction, he maintains the absolute necessity of the operator's first "putting out a hatful of eyes," a statement utterly devoid of truth; inasmuch as we have repeatedly seen the operation of extraction performed, by pupils for the first time on the living subject, with complete success. The following extract affords a fair specimen of the author's style, which is always homely, often inaccurate, and generally very confused. He is describing what is termed "the author's (Mr Neill's) process" of reclination :—

The operator, while he fixes—say the patient's *left* eye with his (the operator's) left hand—the process is done thus: and I am supposing that the speculum is not used. The second finger raises the upper eyelid, and fixes it against the eyebrow. The first finger depresses the lower lid. The eye is thus firmly enough fixed: its outer portion is free to be punctured, and there is no impediment to a thorough inspection of the pupil. A *flat* straight needle, with two cutting edges running to a point, is carried, at the eighth of an inch from the cornea, towards the very centre of the eye through the sclerotic. The needle must not be poked into the lens. As soon as it is judged to be behind the *iris*, it should be gently forced to the anterior of the lens, and posteriorly to the iris, *forwards* toward the pupil. Do all gently. Let there be no abrupt or sudden motion. The point of the needle seen in the pupil is now to be depressed, and *pressed down* the face of the lens to its lower edge, and carried back *below the lens* right into the vitreous humour. The needle is then moved horizontally backwards and forwards, so as to cut a free course in the vitreous body for the passage of the lens. All this is a *continuous*, momentary, and essential movement. Attempted otherwise, a laceration of the vitreous capsule is not easily accomplished; and should the membrane be tough, and not lacerated, the lens may be turned topsy-turvy, and tilted into the anterior chamber.

The needle having made its way through the capsule of the vitreous humour, now comes back to the anterior of the lens. Commence pressure with it, to cause displacement, by resting heavily on the anterior portion of the lens. The lens now moves downwards. The needle presses down also, and the lens begins, technically, to *recline*: that is to say, it turns backwards.

The needle ought to get on the (now) top of the lens; and away it goes, launched as it were into the vitreous humour. Its position is supine, offering its whole flat surface opposed to the vitreous humour. It cannot rise up; its flatness is also opposed to the retinal portion of the vitreous humour.

Such is my favourite operation for the cure of Cataract by Reclination—not as hitherto described, confusedly or with needless elaboration in books. In homely language, I will pit hundreds of cases of *Reclination* which I have successfully performed, against the very best cases of *Extraction*; and the eyes of my patients shall show as little injury surgically produced, *aye and less*, than the optics of those who have had the rare good luck to have been "touched off" by the most dexterous "Extractor."—p. 184.

Leaving the reader to discover Mr Neill's peculiar property in this operation, we pass to the concluding chapter of the book, entitled "Compendious Remarks illustrative of the Inhalation and Use of Chloroform and Æther." As the application of these agents in ophthalmic surgery is nowhere alluded to, we do not see what object Mr Neill could have in introducing this chapter, unless it be

to afford him an opportunity of preferring the following *modest* claim :—

As I have put in some claim as an humble pioneer towards its complete discovery—a discovery which unquestionably took place in America—I will state on what grounds I rest my title to be considered as having aided that consummation. Eight years ago, I published a work on the ear ; and in 1841 the same work was published in New York, Philadelphia, and *Boston*. I gave a regular diagram of an apparatus for the administration of *Æther* by the *eustachian tubes* (air-passages from the throat) for the cure of some forms of deafness, and for the relief of *painful affections* of the inner ear by the induction of *Æther-vapour*. Well, from Boston, in 1846, we have a reflex importation (matured certainly to a climax, entitling the party to the just credit of a complete and triumphant discovery)—to the effect, that *Æther-vapour* will not only soothe pain, but also produce complete insensibility to pain.

It is strange if a thousand copies of the fourth edition of my little work, printed by Mitchell and Company, of Liverpool—regularly shipped and entered through the Customs, and extensively circulated in America, from the towns specified in its title-page—it is strange that such a pamphlet (telling of the very agency, and declaring the efficacy of *Æther-vapour* in relieving pain), should have been five years in Boston, and yet that its suggestions apparently should not have been seen—at any rate not acknowledged—by the new discoverer.

But we must now dismiss Mr Neill's book, in regret that the rich field of observation which the author enjoys had not fallen to one capable of turning it to better account. We really hope, for the sake of the Liverpool public, that the motto which he is so anxious to assume, "*Deus nobis hæc otia fecit*" (see Dedication), may be speedily realized in a sense which he appears at present not to contemplate.

Scriptural Authority for the Mitigation of the Pains of Labour by Chloroform, and other Anæsthetic Agents. By PROTHEROE SMITH, M.D., Member of the Royal College of Physicians, &c., London : 1848. 8vo. Pp. 52.

WE regret the cause of the appearance of this pamphlet, while, at the same time, we recommend it to our readers, as being executed in an honest and worthy spirit towards those whose scruples it is intended to remove. That such scruples should have arisen with the public, especially on this side of the Tweed, among minds strongly preoccupied by peculiar theological ideas, and little accustomed to practical or logical considerations, we were prepared to hear ; but that these views should be shared extensively by men whose practical energies are daily employed, whether chloroform be used or not, in the "mitigation of the pains of labour," and indeed pain of every sort, does indeed strike us with unaffected surprise. The whole question is, to our view, admirably disposed of, as far at least as the profession is concerned, by the following argument of Professor Simpson, in a letter to the author of this pamphlet, which is by no means its least interesting part :—

The accoucheurs and surgeons among you who object to the use of chloroform, on the ground that it goes, in their opinion, against the object and end of the primæval curse upon woman, strangely forget that the whole science and whole art and practice of midwifery is, in its essence and object, one continuous effort to mitigate and remove the effects of that curse. By warm baths, aperients, regulated diet, &c., they attempt to destroy the intensity of the approaching pains and penalties of childbirth;—during labour, they use counter-pressure on the back, to relieve the intense pains there; they use unguents, perineal support, venesection, &c. &c., to ease the pains and insure the safety of the mother. * * * To relieve our patients, however, by our interference, a little, and a little only, is assuredly, in a moral and religious point of view, just as sinful as if we succeeded in affording them complete relief from suffering. The *principle* of interference is not altered by the *degrees* of relief afforded being more or less, greater or smaller.

Littell's Living Age. Reprint of the Report of the Trustees of the Massachusetts General Hospital; with a History of the Ether Discovery, and Dr Morton's Memoir to the French Academy. Edited by R. H. DANA, Jun. March 1848. Boston. 8vo. Pp. 46.

WE notice this pamphlet simply to place on record the conclusion arrived at by the authors of the report, viz. that to Dr Morton belongs the honour of having discovered the power of ether to prevent the pain of surgical operations. Those who are curious to know all the particulars of the painful controversy between Drs Jackson and Morton, will find abundance of detail in the pamphlet before us. The authors have spared no effort to obtain accurate information, and appear to have discharged their duty most faithfully. We may therefore consider this disputed question as definitively set at rest.

Outlines of Lectures on the Nature, Causes, and Treatment of Insanity. By SIR ALEXANDER MORISON, M.D., &c. Edited by his Son THOMAS COUTTS MORISON. Fourth Edition, with Plates, and much Enlarged. 8vo. London: 1848.

THE work of Sir Alexander Morison on insanity is well known, and we can confidently recommend this fourth and improved edition as a work in every way well calculated for those who are commencing the study of the subject. It is written in a peculiarly easy and interesting style, and, without being prolix, contains a full description of the different forms of insanity. Some of the sketches at the end of the volume are very characteristic.

We are of opinion, that the subject of mental diseases is one which deserves the careful consideration of the histologist, and that great fame is to be attained by any one who, with a good previous knowledge of the tissues, will patiently examine into the state of the ultimate structure and composition of the brain in cases of insanity. All our present notions of the morbid anatomy of madness must be

fallacious, for what can be more absurd than speculating about the changes of a tissue, the components of which are so minute as to be invisible to naked sight. Great caution and much labour are required, however, to arrive at trustworthy results, and no one ought to undertake the task who is not at the same time an accomplished pathologist and a skilful microscopic observer.

A Treatise on the Practice of Medicine. By GEORGE B. WOOD, M.D., &c. In two volumes 8vo. Philadelphia: 1847.

THIS work consists of two large volumes, giving an excellent summary of the prevailing opinions and orthodox rules concerning the practice of physic. We do not expect that the doctrines of modern pathology can as yet be brought to influence our treatment of disease, and still less that men of experience should adopt them into their systematic works. For this, as for all good changes, time is required. In the mean time, we can say that Dr Wood's work exhibits great judgment, and may be safely consulted by the student and junior practitioner.

Part Third.

MEDICAL NEWS.

EDINBURGH OBSTETRIC SOCIETY.

MEETING V.—(*Continued from p. 134.*)

ON THE MODE OF APPLICATION OF THE LONG FORCEPS. BY PROFESSOR SIMPSON.

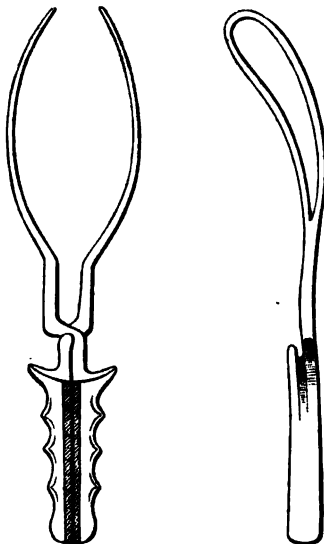
WHEN the head of the child becomes fixed in the brim of the pelvis, and the powers of the uterus fail in propelling it through that aperture, one of two modes of instrumental delivery is usually resorted to,—namely, either 1. Diminution of the infant's head by perforation and craniotomy; or, 2. Extraction of it by the long forceps.

Craniotomy is the preferable operation where the child is dead, and where the pelvic brim is much contracted, or the child's head very much above the average size. But besides being always necessarily fatal to the child, craniotomy is an operation by no means without danger to the mother;—one in every five mothers (according to Dr Churchill) dying, upon whom it is performed. And in most schools of midwifery, it is now, consequently, much more rarely practised than heretofore. Still many authors and practitioners, as Drs Collins, Johnson, Lee, &c., appear to resort to it in all cases in which the bulk of the head is unable to pass the brim, and reject entirely the other alternative of the long forceps.

The long forceps afford in these cases the best chance of life to the child, and when used with proper views of their mechanism and mode of application, their employment, as is often seen in Edinburgh practice, is by no means so difficult and dangerous as some authorities seem to believe. Misapprehensions on these points are perhaps the cause why they are not more frequently used. They differ from the short forceps in some points of construction, but more particularly in regard to their mode of application and working. They differ for example in their length; in the shanks being parallel for some distance beyond the lock (an indispensable point in order to prevent them injuring the outlet); in their blades being curved; and in the part intended to embrace the

head being sufficiently long and large. The instrument which Dr Simpson has frequently and successfully

used (See *Woodcut*) during the last five or six years, has some additional peculiarities. The blades are the same as Dr F. Ramsbotham's, but scarcely so much curved. The lock is Smellie's, but with knees or projections above it of such size as to prevent the blades readily unlocking in the intervals between the pains, thus giving it the fixed character of the locks of Levret and Brunninghausen's instruments, without their complexity. The joints are made so loose as to allow of their lateral motion and overlapping to a very considerable degree, thus facilitating their introduction and application. And, lastly, the handle is that used by Naegele and other German accoucheurs, viz. with transverse knees or rests below the lock for one or two of the first fingers of the right hand to drag by, the long forceps being only properly used as an instrument of traction, not of compression. In addition, the handles are grooved and marked on the anterior side, to distinguish that from the other side when the blades are within the pelvis. The following are some of the dimensions of Dr Simpson's forceps:—Length of the entire instrument, 13½ inches; length of handle,



Dr Simpson's Long Forceps.

including the lock, 5½ inches; length of shank from the lock to the commencement of the curve of the blade, 2½ inches; length of the blade, 6½ inches; extreme breadth of blade at 1½ inch distance from the point of the instrument, 1½ inches; length of the fenestra, 4½ inches; extreme breadth of the fenestra, 13 lines; breadth of the groove of the lock, ¾ inch; thickness of the shank to fit in the groove of the lock, ¾ inch; extreme distance between the blades at 3 inches distance from the point of the forceps when locked, 3 inches; distance between the points of the two blades when locked, 1 inch.

In their modes of application, the long forceps differ from the short. The short forceps are applied always to the lateral surfaces of the child's head, in whatever position the head may be. Generally, the long diameter of the head lies in the right diagonal diameter of the pelvis; and, consequently, the short forceps are placed in the opposite or left diagonal diameter; or, in other words, at right angles to the long diameter of the head. The mode in which the long forceps ought to be applied, and are really applied in practice, has given rise to considerable difference of opinion.

If the long forceps are ever for inertia, hemorrhage, or other such complications, in any case applied while the head is passing through the brim, and the brim and head are natural in size, the instrument may be perhaps applied, like the short forceps, directly to the lateral surfaces of the child's head. But the common reason for the employment of the long forceps, is morbid contraction of the brim of the pelvis in its most general form, and from its most general cause, viz., in the conjugate or antero-posterior diameter, from projection forward of the promontory of the sacrum. How are the long forceps applied when used in this, the case in which they are most generally had recourse to in practice? It is first requisite to state, that under this complication the child's head is found situated in the brim, with its long or fronto-occipital diameter lying in the transverse diameter of the brim, or with the forehead looking to one ilium, and the occiput looking to the other. In other words, the long diameter of the head is not placed, as usual, in the right diagonal diameter of the brim, but

more in its transverse ; for where the promontory of the sacrum forms a morbid projection, the transverse forms the longest diameter of the brim, and, consequently, the one in which the child's head comes to be placed by the uterine efforts. The face or forehead looking to the ilium, and the occiput to the other ilium, the lateral surfaces of the child's head come to be compressed between the protruding sacral promontory and the interior of the symphysis pubis. Now, in seizing the head in this case, some authors aver, that,

1st, *The blades of the long forceps are placed, as in applying the short forceps, on the lateral or aural surfaces of the child's head, and consequently with one blade in front of the sacral promontory, and the other behind the symphysis pubis.* Burns, Dewees, &c., speak thus of applying the long forceps in the conjugate diameter of the brim ; and Dr Churchill has published a woodcut representing this as the actual method of their application in practice. But its application in this position is impossible in the very cases in which the long forceps are generally required, viz., where the conjugate diameter is contracted, for there is not room for the additional thickness of the blades of the instrument ; if applied, they add to the thickness of the head in that one diameter and place in which it is already too thick and large ; their pressure would greatly endanger the urethra and bladder in front, and the soft structures placed over the promontory of the sacrum behind ; and they could not thus be placed in the axis of the brim in consequence of the pressure of the perineum upon the instrument below. Other authors aver, that,

2. *The blades of the long forceps should be placed over the occiput and forehead or face of the child, and consequently in the transverse diameter of the brim.* This is the view of their mode of application taken by Deleurye, Davis, &c. &c., and approaches much nearer the reality than the former opinion ; but that it is not strictly true, is shown by the marking of the place of application of the blades of the instrument after the child is born, and by a more attentive consideration of the mechanism of such labours. One blade is found to have been placed behind one ear, and the point of the other to have pressed upon the side of the forehead, temple, or region of the eye ; but these would not be the places of the markings of the blades if they were applied in the transverse diameter, upon a head placed directly transverse. Dr Ramsbotham has published a beautiful plate of the mode of application of the long forceps, and has given an excellent chapter on the subject in his work on midwifery. He correctly represents in the plate the anterior blade as placed upon the side of the forehead and eye-brow ; but in order to give this view with the forceps placed in the transverse diameter of the brim, he has been obliged to represent the face as turned backwards, whilst in reality, in morbid contractions of the conjugate diameter of the brim, it is actually turned laterally ; and he places the long diameter of the blades of the forceps so as to traverse the right oblique instead of the left oblique pelvic diameter.

3. *The blades of the long forceps should, I believe, be placed*



Mode of Application of Long Forceps.

obliquely upon the child's head,—one, the posterior, over the side of the occiput; and the other, or anterior, over the side of the brow or temple, and consequently should be situated in the oblique diameter of the brim. (See Woodcut).—The markings on the child's head after birth always show this mode of application of the instrument: when properly applied upon the mother, and when their situation relative to the pelvis is examined, they are found to have assumed this position; and in experiments with the instrument (when the head of a dead child is fixed in a pelvis with a contracted brim), this is the position and relation which the instrument will be seen to assume with relation to the infantile head and maternal pelvis. Besides, in thus placing the instrument, while we incur less danger of injuring the urethra and other important parts, we place the blades of the instrument in exactly those parts of the pelvic circle where there is least pressure, and consequently most room for them. It is apparently in consequence of misconception on this point, that some authors have come to prefer the use of the perforator to that of the long forceps. Dr Collins, for example, argues that when the head is detained in the pelvic brim, the brim "measuring little more than three inches from pubis to sacrum," there cannot possibly be space for the long forceps even were the bones denuded, seeing that the blades of the smallest sized forceps used in Britain, even when completely closed, measure from $3\frac{1}{4}$ inches to $3\frac{1}{2}$. "How (he adds) is it possible with the forceps to drag a child through a pelvis where there is not space, except by force, to introduce, as is commonly said, a straw, or where the smallest flexible catheter cannot be passed in some instances into the bladder?" These and such opinions proceed on the erroneous idea, that the long forceps are to be applied, within the pelvis, at the parts or in the diameter in which the pelvis is *most* contracted, and they suppose that the head when fixed in the pelvic brim fills completely the *whole* circle of the brim. The usual shape of the morbidly contracted pelvic brim is cordate, or rather elliptico-cordate, but the child's head is not of this shape; it is ovoid, and consequently when applied to the cordate brim leaves unoccupied spaces. The most unoccupied spaces before and behind are at the extremities of the oblique diameters of the brim, where sufficient room is left for the passage of the blades of the forceps, and in these points they are passed when properly applied.

[The preceding observations were illustrated by various experiments with the forceps, and by showing its mode of application upon a dead child's head placed in a contracted pelvis. A number of cases illustrative of the use of the long forceps were mentioned by Dr Simpson, Dr Moir, and other members; and the cases in which turning was preferable pointed out.]

CHLOROFORM IN A CASE OF PUERPERAL CONVULSIONS. BY DR CUMMING.

Dr Cumming related a case of puerperal convulsions coming on after delivery. The patient was a young girl, seventeen years of age, who had been seduced; but, during her short residence in the Maternity hospital, had not been observed to be peculiarly depressed in spirits, or otherwise affected in mind. The labour, all circumstances considered, was unusually short and easy; all the three stages having been completed in six hours. She was delivered at four A.M. At nine A.M. the first convulsion occurred. Venesection was attempted by the house-surgeon; but, in consequence of the resistance and convulsive struggles of the patient, unsuccessfully; the other usual remedial measures, however, were put in operation. I saw her about one o'clock, along with Dr Ziegler and others. She was then with considerable difficulty bled at the arm to sixteen ounces, and as much more was taken away from between the shoulders by cupping; the other remedial measures were also persevered in so as to affect the pulse and general hyperemic condition very decidedly, but with no improvement in the convulsive seizures, which recurred at frequent intervals. During the intervals she was occasionally sensible, so far at least as to indicate that she understood what was said to her. Professor Simpson saw her in the evening, and recommended further depletion by cup-

ping, which was done, but without any effect in rendering the convulsions less frequent or less severe. It was then suggested that chloroform should be administered. The effect of its application was speedy and decided. Though she survived six days, every effort on the part of the system to throw itself into convulsion was checked with ease; so that nature was not so soon exhausted as it would otherwise have been, and other remedies could be applied. It should also be mentioned that the interval of most perfect consciousness was after the chloroform had been applied for some time. Appearances were at this time so favourable as to encourage the hope that she would struggle through. Dr Cumming in conclusion remarked, that, though the issue of the case was unfortunate, nothing could be more satisfactorily established than the power of the chloroform to subdue convulsions, and thus, even when it did not (as in other reported cases it appears to have done) exercise a direct beneficial influence over the disease itself, it prevented the excitement and consequent exhaustion of the system, and admitted of the application and administration of such measures as the case otherwise indicated.

Dr Thatcher stated that he had observed that patients who, when not pregnant, were liable to epilepsy, were frequently the subjects of puerperal convulsions when in labour.

Dr Simpson had on a previous occasion attempted to show that one of the most common predisposing causes of puerperal convulsions was albuminuria, or rather some morbid state of the blood dependent upon albuminuria,—a doctrine which he had taught for the last seven or eight years (see *Monthly Journal* for 1843, foot-note p. 1015), and which Dr Lever had studied and successfully illustrated. He believed this diseased condition of the blood to produce a preternatural excess of irritability or polarity of the nervous system, and more especially of the spinal system of nerves; and in this way a morbid predisposition in it to convulsions under forms and degrees, irritation in the uterus, or bladder, or intestinal canal, &c., that would be incapable of producing this convulsive effect were not the polarity of the nervous system for the time being highly exaggerated. Dr Todd of London had shown last year, that, when the polarity of the nervous system was exaggerated in the lower animals by the exhibition of strychnia, the convulsive tendencies resulting from the strychnia could be so far subdued and decreased by making the animals inhale ether. The ether reduced the excessive polarity and consequent convulsions. And the application of chloroform, in the same way in puerperal convulsions, had been tried with success by Dr Wilson of Glasgow, Mr Fearn, Clifton, &c. Dr Simpson had suggested that the organic conditions of the kidneys causing albuminuria, and predisposing to convulsion in puerperal women, was probably often one of *Stearosis*, produced by the previous circulation of the butyraceous or fatty principles of the milk in the mother's blood before delivery. In the present case, one portion of the kidney after death distinctly presented the stearoid or fatty degeneration.

Dr Peddie read a paper "on the mammary secretion; its character, chemical and structural; the value of the microscope in the study of it; its existence an important sign of early pregnancy; and its pathological changes; with hints regarding lactation and the choice of nurses."—[Dr Peddie's paper appeared at length in last No. of the *Monthly Journal*.]

MEETING VI. June 14.—DR MOIR, Vice-President, in the Chair.

CASES OF PROLONGED LACTATION.

Dr Moir mentioned the case of a married lady, who, from certain reasons, was unable to nurse her children, but in whom the secretion of milk was after each confinement unusually protracted, notwithstanding the means used to discuss it. Starvation, purgatives, diuretics, diaphoretics, and alteratives, as internal remedies, and local astringents of various kinds were used, without in the least diminishing the secretion, or having any effect further than that of weakening the patient; so that latterly the only means employed were of a

tonic nature, to sustain her strength. The secretion continued from one confinement till about the third month of the succeeding pregnancy, after which it almost ceased; after her second confinement it continued eighteen months; after her third, twenty-four months; after the fourth, twenty-five months; after the fifth, about twenty-four months, when she had a miscarriage, and since then has had no children. A circumstance that rendered this case peculiarly distressing was, that after her first confinement she suffered from a severe mammary abscess, which, by the practitioner then in attendance, was opened close to the nipple. From this wound the milk continually flowed, and as it never healed up, it was impossible to receive the milk from this breast into any convenient reservoir; so that the lady was kept in a constant state of discomfort, her dress, notwithstanding the use of oiled silk and M'Intosh cloth, being completely saturated with milk. The skin over the abdomen and left side was from the same cause much irritated, and in warm weather partially excoriated.

Dr Peddie knew of a woman having nursed uninterruptedly for three years.

NEW FORM OF PESSARY.

Dr Weir exhibited a pessary for prolapsus of the uterus, which he considered useful from its cheapness, its lightness, and the facility of its introduction and retention. It was composed of a slender stem of light wood, from two to three inches in length, surmounted by a shallow cup (see *Figure*), on which the cervix of the uterus rested. The lower end of the stem was furnished with a small knob or button, which, when the instrument was *in situ*, rested on a simple T bandage. This form of pessary had been used for some years by *Dr Weir*, and by many others, with the best results.



Dr Weir's Pessary for Prolapsus Uteri.

CASE OF PLACENTA PRÆVIA TERMINATING FAVOURABLY TO THE MOTHER AFTER RUPTURE OF THE MEMBRANES. BY DR E. SKAE.

The subject of the case was a Mrs Orr, the wife of a mechanic, and it took place in her third confinement. In her two previous labours, the presentations were preternatural: in the first the breech presented, and in the second the feet. On both occasions, her delivery was successfully accomplished. At these labours, as well as the one under consideration, she had the assistance of a midwife, who afterwards called me in to complete them. She sent for me on the 26th November 1841, in consequence of a sudden and rather alarming attack of flooding, unattended by pain. She stated to me that she expected confinement in the course of a fortnight; that she had been once or twice threatened with flooding during the two or three weeks preceding. On examining the os uteri I found no indication of impending labour, and the hemorrhage had ceased before my arrival. I enjoined absolute rest, and prescribed repeated doses of opium with acetate of lead. The following day she felt quite well, and on the third day I found her moving about, and stating that she felt perfectly well. On the morning of the 3d of December, five days after, I received an urgent call to visit her without delay. I found, on my arrival, that the midwife had been with her for upwards of two hours. Before mak-

ing any examination, I was informed by this attendant, that, during the time just mentioned, and for some time previously, there had been considerable flooding, that the os uteri was considerably dilated, but that it was occupied by a soft mass, which she had latterly endeavoured to pull away, believing it to be a false conception, but that she had speedily desisted in her efforts to do so, in consequence of increased flooding; that she had kept the passage plugged, and had recently administered the ergot of rye. On withdrawing the stuffing which had been introduced into the vagina, and some coagulated blood, I discovered the upper part of the passage occupied by the placenta, a portion of it projecting considerably into the vagina, and somewhat lacerated, the result, no doubt, of the midwife's interference. On carrying my fingers round this mass, I found the uterine aperture well dilated, but its whole area occupied by the placenta. I could readily perceive, after careful tactile examination, that although the whole circumference of the opening was filled by the placental mass, prior to the accession of labour, its centre must have been implanted not exactly over the os uteri, but rather posteriorly; for by carrying my finger between it and the cervix anteriorly, to the extent of an inch or more, I could reach the membranes, or what I supposed to be them. I saw no alternative but to follow the only steps at that time sanctioned in obstetric practice, viz. to perforate the membranes, if they could be reached, and deliver by turning, should the nature of the presentation require it. After carrying my hand into the vagina, I passed two fingers carefully upwards and forwards, between the placenta and the cervix anteriorly, until I felt satisfied that I was touching the membranes, these I perforated, but had no sooner done so than a gush of liquor amnii was followed by the immediate and forcible descent of the foetal head into the brim of the pelvis, and, by the continued and energetic contractions of the uterus, it speedily engaged in its cavity, the placenta being thrust backwards against the sacrum. Within ten minutes after the membranes were ruptured, a dead foetus was expelled, and the placenta almost immediately followed. There was no subsequent hemorrhage, and the recovery was uninterrupted by a single unfavourable symptom.

TRIAL OF WILLIAM HETHERTON, CHARGED WITH THE MURDER OF HIS WIFE BY
WOUNDING HER IN THE GENITALS.

WILLIAM HETHERTON was tried at Edinburgh, July 24th, before the Lord-Justice Clerk, Lord Cockburn, and Lord Wood. The Solicitor-General and Mr Crawford conducted the prosecution; Mr Moncreiff and Mr Graham the defence.

Hetherton was a blacksmith, and had been married to the deceased little more than nine months. At the time of her death she was about eight months gone with child. Both were addicted to drinking, the wife in particular. She had pawned his clothes to get liquor, and he had sometimes beaten her in consequence. On the evening before the death of the deceased, between eight and nine o'clock, a noise of quarrelling was heard by the neighbours, and some hours later moans, as of a woman in pain, proceeded from the house. About nine o'clock Hetherton went to his sister's house, about fifteen minutes' walk from his own, where he remained about half an hour, and told her that his wife was in liquor, and that he had trailed her from the kitchen to the inner room, and laid her on the sofa. Between the time when the accused left his sister's house at half-past nine that evening, and four o'clock the next morning, nothing is known of what he did except from his judicial declaration. In it he states, that having gone home from his work that evening between seven and eight o'clock, he found his wife lying on a chest in the kitchen drunk—that he pulled her off the chest, and being unable to carry her on account of her weight, dragged her to the inner room, placed her on the sofa, and then left the house to go to his sister's—that he returned from

his sister's about ten o'clock, and did not then go near his wife, but went straight to bed in the kitchen—that about half-past three in the morning he was awakened by his wife calling out, "Rise, Willie, rise!"—that he arose and found her lying on the floor beside the sofa—that she asked for water but could not take it when he brought it, and did not answer when he asked what was the matter—that he put a pillow under her head and threw a blanket over her, then went to bring his sister-in-law. From his sister-in-law's evidence it appeared, that the accused came to his brother's house about four o'clock in the morning, asking her to come along with him as his wife was taken ill. His sister-in-law wished him to go and get a doctor while she dressed herself—he, however, insisted that she should go and see his wife first, and waited till she was ready. On the way to his house she again told him that nothing could be done without a doctor, while he still urged that she should go and see his wife first. She, however, declared that she would not go without a doctor, and then he consented to her calling for Dr Burn in their way. Hetherton then went on before, and she followed shortly with Dr Burn. They found Hetherton waiting for them near his house, and they all went in together, and found Mrs Hetherton lying on the floor on her left side, with her back to the sofa, and her head a little beyond the end of it, not undressed, and with a blanket over her. Neither Dr Burn nor the sister-in-law appear to have observed any pillow under her head. Dr Burn deposed that he found her quite cold, still breathing, but pulseless and evidently dying—that she was lifted into bed, and in a few minutes expired. Dr Burn had come under the impression that it was a case of premature labour brought on by intemperance, and nothing suggested to him any suspicion of violence. He believed at the time that death had been caused partly by hemorrhage, partly by extreme intoxication. He had heard before that the deceased was much addicted to drinking. He had known the accused for a long time. He observed a bruise on one of her hands, which Hetherton ascribed to a fall. When he first saw her on the floor, there was no appearance of any derangement of her dress. There was some blood on the carpet near her feet, and if that blood had come from the lower part of her person, she must have been moved afterwards towards the inner end of the room; if she had fallen from the sofa, she must have been moved in the same direction after she fell. He did not examine her person or her clothes particularly, having no suspicion of any thing being wrong, but he felt there was blood on her under-clothes. He applied his hand to the abdomen, and examined the vagina, in which he found no coagulated blood. The sister-in-law described the position of Mrs Hetherton when first seen, and also the place of the blood, in exactly the same terms as Dr Burn; and from her statement it appears that the quantity of blood was not inconsiderable. She saw some clotted blood in a chamber-pot beneath the sofa, and in the kitchen a sponge in a soap dish, with what appeared to be fresh blood upon it. The clothes taken from her person were put into a tub of water. Two females who assisted the sister-in-law in dressing the dead body, corroborated her evidence as to the blood on the clothes, and on the carpet, and spoke to bruises on the arm of the deceased. Preparations were made in the course of the same day, with some haste, for the funeral, and in the evening the body was placed in the coffin—but as information had been lodged with the authorities as to the noise of quarrelling heard the previous evening, Dr Weir and Mr Glover were directed to examine the body of the deceased. The essential part of their report is as follows:—

"The body appeared to be that of a person about thirty years of age. *Externally*—With the exception of an ecchymosed spot at the outer angle of the left eye, the head presented no marks of violence. Both arms were covered with numerous marks of recent and old contusions, particularly about the left elbow and forearm; the cuticle was also abraded in one or two places of the left hand. An ecchymosed patch of considerable size was also observed on the left side of the body, a little below the navel. The genital organs were wet

with blood, and stained the shift to a considerable extent. The left labium was greatly enlarged, being three or four times larger than natural; the right was normal both in size and appearance. Both legs, from the hips down to a little below the knees, were covered with marks of contusions, all of which, when cut into, presented an ecchymosed appearance. *Internally—Head.*—On detaching the scalp, we found two ecchymosed patches on its inner surface, one on the fore part of the head, the other on the back part—the first was circular and about the size of a half-crown, the latter more irregular in shape, and much longer. Upon removing the skull cap and dura mater (the external membranous covering of the brain), a serous effusion of a straw colour was observed under the arachnoid (middle membrane). The effusion was most abundant over the posterior hemisphere of the brain. The veins, sinuses, and substance of the brain itself, were almost destitute of blood, giving the organ a most blanched appearance. *Chest.*—Lungs healthy in structure, and not collapsed. Heart a little larger than usual, the substance pale and bloodless; all its cavities nearly empty.

“Abdomen and Pelvis.—Viscera healthy, but perfectly blanched, particularly the liver. The uterus (womb) contained a foetus about seven or eight months old, and was healthy in all its structures. On cutting into the enlarged labium already referred to, its substance was found to be infiltrated with serum and coagulated blood. On its inner surface a small portion of the mucous membrane was abraded, similar to that remarked on the left hand. Upon separating the labia, and pushing back the nymphæ, we observed a ragged wound situated immediately to the left side of the orifice of the urethra. The dimensions of this wound were about three quarters of an inch long, half an inch broad, and three quarters of an inch deep. It easily admitted the point of the forefinger, which, when introduced, could be moved about in a cavity of much greater size than the external opening. In the walls of this cavity, and on the parts adjoining, we found a small quantity of coagulated blood. From the above *post mortem* appearances, we are of opinion, that death was occasioned by the profuse hemorrhage from the wound in the genital organs, and the other injuries described.”

A number of smith's tools were found in the house, several of which the medical witnesses were of opinion might have sufficed to produce such a wound. The police-officers on searching the house took possession of the carpet and crumb-cloth of the inner room, through both of which the blood had penetrated to the floor; they observed blood upon the tongs and shovel in the same room, on the ends used in the fire; in the kitchen they found the sponge before referred to, with blood upon it, and also a stick with some spots of blood upon it, and under the bed in the kitchen they found a striped shirt, one of the sleeves and one of the sides of which were stained with blood. Hether-ton, in his declaration, acknowledged that the shirt was his, but could not account for the blood upon it, unless it might have been put amongst his wife's clothes, or had been stained in assisting to put his wife into bed; but he could not say whether he wore the shirt on the night in question or not. He said, that three or four weeks before, his wife had bled a good deal from the private parts after some days' hard drinking, and he supposed that the loss of blood at the time of her death was from the same cause. As to the bruises on her body, he acknowledged that he had beat her about the upper parts of the body that evening both with his hands and with a stick, not with the purpose of doing her any serious injury, but to make her confess what she had done with a shirt which he missed; and being shown the stick before referred to, he said that that was the stick he had used, and that it was broken over her—he further said, it was after he had dragged her into the inner room and laid her on the sofa, that he beat her, and that she cried “murder” then, and also at the time he was dragging her—further, that he saw no marks of bleeding about his wife when he put her upon the sofa, or before he left the

house about nine o'clock to go to his sister's, but that he saw blood at her feet before he went for his sister-in-law in the night-time—that the marks on the tongs and shovel were not blood, but rust produced by drops of water falling upon them when water was taken from the cistern which stood near—and, being shown the sponge, he declared that he had not used it on the night of his wife's death.

After the declaration, from which extracts have just been given, was read in court, Drs Gairdner and Douglas MacLagan were examined in exculpation. Dr Gairdner deposed, that he had examined the report of Dr Weir and Mr Glover, and was of opinion that the wound near the urethra might have been inflicted by a kick given by the foot from behind, supposing the clothes were removed. It might also have been produced by falling on a hard substance under similar circumstances. Dr Douglas MacLagan gave similar testimony, and added that the laceration might have been produced by any blow impinging on the pubis, though the instrument did not reach the seat of the wound. This, however, was merely a theoretical opinion.

After being charged by the Lord Justice-Clerk, the jury remained in deliberation for an hour and a half, and then by a majority found Hetherton guilty of culpable homicide (manslaughter). He was sentenced to be transported for life.

The verdict shows, that it must have been the opinion of the majority of the jury that Hetherton, by the injuries which he inflicted, had no intention of taking away the life of his wife. In short, they find that he did not, or at least that there is no sufficient evidence that he did, deliberately inflict the wound in the genitals of which she died, with the knowledge that such a wound would probably cause death. As we despair of ever seeing more complete evidence in any instance of this insidious kind of murder, the melancholy conclusion is, that this is a crime which may be committed under cover of very slight precautions against detection, without subjecting the offender to the proper judicial penalty. This woman unquestionably died of hemorrhage—not of that kind of hemorrhage which might naturally arise from mere ill usage in an advanced state of pregnancy, but from hemorrhage the result of a wound that must have taken place but a few hours before. Be it so that such a wound might be the effect of a kick, it could hardly have been other than a kick employed deliberately with the expectation of causing death. If a man uncover his wife's person while she is insensible from intoxication or previous ill-usage, or the joint effect of both, and place her in such a position as shall freely expose the orifices of the internal parts, and then deliberately kick her in those parts, thus producing a rent capable of giving rise to a fatal hemorrhage, he is as much guilty of murder as if he had made a gash there with a razor. In the circumstances of the case before us, it is inconceivable that a mere *random* kick could have caused the wound. The woman was dressed as usual—she had no stays, but she had a printed gown and two petticoats over her chemise. Whether we suppose her standing, or on the sofa, or on the floor where she was found, such an effect in the place described of a *random* kick must be pronounced impossible. If, then, Hetherton was not guilty of murder, the only alternative (since we believe that the wound was deliberately inflicted) is the extremely improbable one, that he did it with the minor intention of injuring her, but not of causing death; unless we resort to the still more violent supposition, that the deceased inflicted the wound herself.

REPORT OF THE COMMITTEE ON POOR-LAW MEDICAL OFFICERS.

WE regret that we have only space for the concluding proposals of this excellent document. They are as follows:—

" 1st. That in the opinion of your memorialists, it is essential that the payment of medical officers should be by a fixed salary, exclusive of fees for midwifery and important surgical cases.

" 2d. That the amount of salary might justly be fixed, either by an estimate of the average of cases attended during a series of past years, considered in connexion with the area of the medical district, or by the payment of a certain sum per head on the population, corrected by the consideration of the relative density and poverty of the district.

" 3d. That making every allowance for the difference which must exist in the remuneration of private and public practice, it would not be just that the salary should be based on a less amount than 6s. 6d. per case, with the charge varying with the area.

" 4th. That medical officers of work-houses should be paid a separate salary, based on the average number of inmates, at not less than 7s. to 10s. per head.

" 5th. That it is just and proper, and conducive to the interests of the poor, that an extra payment should be made, as at present, for midwifery and for important surgical cases; and that it appears to your memorialists advisable that the fees should be paid for cases occurring in work-houses, as well as in out-door cases.

" 6th. That to the cases of surgery requiring payment, enumerated in the General Order of the Poor-Law Commissioners, should be added—

" Fractures of the clavicle;

" Fractures of the skull;

" Retention of urine, when requiring repeated introduction of the catheter;

" Severe burns and scalds;

" And that L.3 be paid for compound fracture of the arm.

" 7th. That the payments allowed for midwifery and surgical cases under the General Order of the Poor-Law Commissioners are satisfactory in amount.

" 8th. That in order to remove difficulties in the way of a more equitable payment of medical officers, and considering that half the Union medical expenses are already paid from the Consolidated Fund, it is highly desirable that the whole expenses of the medical attendance on the poor should be removed from Unions to the Consolidated Fund.

" 9th. That it is expedient that there be a Director-General of the Poor-Law Medical Department appointed by Government, and exercising supervision over poor-law medical practice, in the manner of the other public services.

" 10th. That it is expedient that there should be Inspectors-General of Poor-Law medical practice appointed by Government, and acting under the orders of the Director-General.

" 11th. That the Inspectors-General, acting each in a district assigned to them, should examine the Infirmaries of work-houses, inspect the reports of the medical officers, inquire into cases of alleged negligence, &c. &c.; and that matters of disputed payment should be referred to them.

" 12th. That the medical officers of Unions should be appointed by Boards of Guardians as at present, subject to the approval of the Director-General; that their qualification should be as at present ordered, but that all future modifications of the qualifications should be made by the Director-General.

" 13th. That the appointments of all medical officers should be permanent—that is, to endure until they die, resign, or are dismissed for some valid cause.

" 14th. That the Forms of Books or Reports should be approved by the Director-General, and should be as short and simple as is consistent with the requirements of the public service.

" 15th. That the access of the paupers to the medical officers should be made as ready as a due distinction between those who require parochial attendance, and those who do not, will admit. That in doubtful cases where illness exists, and there appears to be temporary destitution only, the Board may grant medical relief by way of loan; that in such cases a fee of _____ be paid by the Board of Guardians to the medical officer. Signed on behalf of the Committee,

" THOMAS HODGKIN, M.D., *Chairman.*"

SANITARY REFORM—THE HEALTH OF TOWNS BILL.

We congratulate the public of England on the progress of this bill, which is now so far advanced that it may be considered as secure. By an amendment in the Upper House it was proposed, that upon the petition of one-tenth of the rate-payers, in any district where the deaths from epidemic, endemic, or contagious diseases exceed the proportion of 20 per cent. of the whole deaths, the General Board of Health may direct a Superintending Inspector to make inquiry as to the sanitary condition of the district, and report to the Board. The Commons alter this amendment in as far as relates to the above test of the necessity of inquiry. The amendment, in the form in which it has passed the House of Commons, enacts that the Board of Health may institute inquiry, when the deaths in 1000 of the population exceed 23 per annum. A conference with the Lords has been appointed to settle which of these amendments shall become law. Considering that the average mortality in England is about 21 in 1000 of population, the latter would bring many of the more unhealthy districts immediately within the scope of the act.

The limitation of this act to England is its greatest imperfection; and, considering the probable advent of cholera, is a subject of the deepest regret. We are afraid that Scotland and Ireland, especially the latter, will be found no better prepared for this scourge, in the event of its arrival, than at the time of the last epidemic. Indeed, it is to be feared, that if famine be superadded to pestilence in Ireland by a second failure of the potato-crop, the visitation may occur under yet worse circumstances than on the former occasion.

Both Scotland and Ireland were promised separate bills at the beginning of the session; but these with many others have been dropped.

THE ROYAL COLLEGE OF CHEMISTRY.

We regret to see, by the report of the annual meeting of the College of Chemistry, that the financial condition of the institution is not flourishing. It had been found necessary, it is said, to incur a heavy debt for the building of the laboratories, &c., and a fund was opened to defray it, which promised well in the beginning, but eventually left the institution with liabilities to the extent of L.2000. In order, as far as possible, to meet those liabilities, the council proposed a *pro rata* subscription among their body. The result was, that several members of the council, and other noblemen and gentlemen, amounting in all to twenty-three, contributed L.50 each towards the object, and six others promised to come forward with a similar sum. By this means they were enabled to pay off the debt upon the building account, amounting to L.1200, which covered every remaining liability connected with the building, with the exception of L.100 due to the architect. The increase in the number of students and their progress are reported as satisfactory; and it was stated that in the laboratory all classes of society are represented. "Working at the same table, may be found a peer of the realm, a medical student, a member of Parliament, and a druggist's apprentice."

CHOLERA AND MAGNETISM.

The *Manchester Guardian*, quoting a letter from St Petersburg, says—"A very important discovery has recently been made here, which clearly proves that the malady is in the air, and that, therefore, quarantines are utterly useless. The air here has had a very singular effect on the magnetic power. Whilst the cholera was at its height, the action of the magnet was nearly neutralized; which, now the disease is gradually subsiding, assumes by degrees its former power. A magnetic block which used to carry 80lb. would, during the worst time of the cholera, not carry above 13lb. Its strength has now in-

creased again to 60lb. The electro-magnetic telegraph at one time would not work." It was also recently mentioned in the *Lancet*, that, during the prevalence of Asiatic cholera in London, there was a cessation of the disease in St Giles's after a violent thunder storm.

STATE OF THE PARISIAN HOSPITALS AFTER THE INSURRECTION OF JUNE.

WHAT strikes you at first entering the wards of the Hotel Dieu and St Louis is the quietness of every thing; the silence and propriety of the *élèves*; the anxiety of the medical man going round for the welfare of the patients, and the information of the pupils, by whom, I must say, I never witnessed greater kindness or attention shown to any class of patients. At the entrance to each ward the *Personnel* and *Reglemens* are posted up; the former containing the names of every one engaged in the ward, medical men, chaplain, matron, nurses, &c.; the rules also not less conspicuous. The pupils, however, are interdicted from stopping in the ward after the surgeon, from perussing the patients, or in any way interfering with them; matters of rather questionable utility as far as the pupils are concerned.

The hour of visiting the hospitals is eight o'clock A. M. The *blessés de Juin* were lying every where that accommodation could be had. Many of the cases were of the most frightfully bad character; fore-arms torn in pieces by balls; hands shattered, and legs literally smashed by cannon-shot; by far the greatest number, however, were balls through the chest and shoulders, and all parts of the body exposed above the level of the barricade. It was quite wonderful to see what nature was doing for an immense number of these; the *Vie Médicatrice* seemed, indeed, all-potent. It was curious, too, to witness the nice parts the surgeons had to play; a word of comfort to one; a smile with another; a little bit of evasion with a third; many of them having, it is known, had more to do in the work of the barricades than they were willing to confess. There was an appearance of conscious misgiving about many of these poor creatures that one pitied. Several of them were wretchedly ghastly beings that an *émeute* always throws to the surface; many to whom death would be a respite from their thousand sufferings. There were also the young fellows of the *garde mobile*, especially at the Hotel Dieu; some of them wounded very slightly, others badly. One should see these fierce young fellows under every aspect to understand them; thundering along a *pas de charge*, or rolling in the grass at Versailles, without care or anxiety of any kind, except perhaps for fighting, and without a wish beyond that of standing sentry with a long gun, nearly twice the length of themselves, at the door of the camp. These nearly all recovered. * * * The general appliances of the hospital were excellent. Oiled silk, lint, charpie, sponges, ices, wines, jellies, and soups in abundance. Much more stress is laid on the article of diet than in our own hospitals. Every thing seemed well managed indeed. * * * Several amputations were performed. Other cases, though of a very grave character, were apparently doing very well under the ordinary treatment. In one case the ball went right through the popliteal space without injuring either nerves or vessels; in another, a man shot in the back of the head, the ball went out of the mouth; both were apparently doing well. In many instances the ball had gone through the shoulder-joint, without any very manifest symptoms supervening. In another case a man was shot through the kidneys,—the ball making two openings,—actually broken in two on the bodies of the vertebræ. In another case, doing well, the entire foot was shattered by a cannon-shot. In another, a very severe injury occurred in the tract of the femoral vessels, without, however, any hæmorrhage; the man seemed as if nothing had happened. The injuries of the lower extremities were, indeed, almost beyond belief. In one singular case of injury of the head, the entire of the coverings of the lower jaw had been swept away, leaving the cartilages of the larynx ex-

posed; there seemed no unusual hæmorrhage. In a severe injury of the upper extremity, the ball traversed the upper part of the shoulder, wounding the spinal column, and producing a want of sensibility and motion; the poor man was alive simply because it had not gone one half inch higher, and cut off the origin of the phrenic. * * * The treatment in all the hospitals is, perhaps, a little too general; what is ordered for a stout *mobile* being likewise ordered for a starving insurgent. All those injuries not requiring amputation are treated by cold applications, and venesection if necessary, followed, after reaction, by emollient cataplasms. There is very little cutting or enlarging of wounds, and a great deal is left to Nature. The relative mortality will, perhaps, prove whether this or the opposite system is to be preferred. In two of the worst cases of wounds of the chest here, the operation for empyema had to be performed.—*Dublin Quarterly Journal, Letter to the Editor by Charles Kidd, M.D.*

THE PARISIAN PHARMACIENS AND THE INSURGENTS OF JUNE.

WE know the means employed by the insurgents either to procure munitions of war or to render their projectiles more efficient. Several pharmaciens in the Faubourg St Antoine, at La Chapelle, and in the Faubourg Saint Marceau, were ordered to prepare powder and gun cotton. One of these, we are assured, did not require much persuasion; the others only complied with a pistol at their head. Besides this generally efficacious instrument of persuasion, the insurgent gentlemen employed an infinity of politeness, and even of good-humour. A pharmacien alleges that he does not know how to make gun-powder. "Oh, *mon cher!*" replies one of the leaders, "if it were how to invent it, I should not address myself to you; but it is only a question of preparing it, and I shall help you. *Recipe*—Saltpetre 75; charcoal 12·5; sulphur 12·5. Mix in a wooden mortar." "But I have no saltpetre." "Bah! I see on the bottle there NITRAS POTASSÆ. We shall manage with this nitrate of potassa." The unfortunate pharmacien is obliged to execute the order. Another tried in a similar way to elude an injunction to prepare gun-cotton, but forthwith they had placed upon his counter all the necessary ingredients, and set his restive spirit to work. The process succeeded marvellously!—*Feuilleton de la Gazette Médicale*, No. 29.

STATE OF MEDICAL EDUCATION IN TURKEY.

[We are indebted to a medical gentleman, at present residing in Constantinople, for the following details. They point to the existence of a state of matters, of which very imperfect accounts have as yet reached this country; and such as, undoubtedly, is at once the proof and the earnest of a new era in the history of civilisation having begun in the East.]

"Military hospitals, on a large scale, are either built or a-building in every quarter of the Turkish empire. There are about one thousand European surgeons attached to the different regiments, two hundred of whom are Jews. The chief professor of the Medical College, Dr Spitzer, is a Jew. He is also one of the physicians in ordinary to the Sultan. By him I was introduced to H.E. the Hakim Bashy Ismael Effendi, the chief physician of the Ottoman empire, who kindly permitted me to visit the different lecture-rooms in the Medical College. It is certainly in a very flourishing condition, considering that it has been in existence only eleven years. The pupils are brought, by order of the Sultan, from all departments of the empire, and are lodged, fed, clothed, and educated at the expense of the government. When qualified as physicians and surgeons, they receive appointments in the army and navy, with salaries of L.200 or L.300, and upwards, according to rank, without distinction of Jew or Gentile. Until lately, however, there were no Jews in this college: not that

the government excluded them, on the contrary, they were invited ; but that people, who have been scattered amongst all nations, yet amalgamated with none, would not send their sons to this medical establishment, even although the most flattering prospects of education and worldly advancement were held out to them. But the government condescended to smooth all the difficulties which stood in the way of the improvement of this section of its subjects. Through its agents, it held interviews on the question of conscientious scruples with the chief Jewish Rabbis ; and the result was, not only the guaranteeing liberty of conscience to the Jews who should enter the Medical School, but the assigning to them a distinct portion of the college, so that they might live separate from the Gentiles, the appointment of a superintendent of their own persuasion to see that their religious duties and services should be strictly observed—also a shokel or butcher of their own ; and, in short, every arrangement was made to prevent their being constrained to do any thing contrary to their conscience. In the language of last year's report of the college authorities,—“ Toutes les difficultés ont été aplanies, et le Gouvernement n'a reculé aucune sacrifice pour exercer aussi son influence civilisatrice sur cette partie des sujets de l'Empire.” The Sultan lately visited the college and presided at the examination of the candidates for the medical degree. When the pupils are first introduced to the college, they are, for the most part, raw, ignorant, and uncivilized. They are at first taught Turkish, afterwards the Arabic and French languages ; next geography, history, arithmetic, and other elementary branches of education, including natural history. They have already a very tolerable museum of natural objects, well preserved and well arranged. A small botanic garden is also attached to the college. After undergoing a thorough elementary education, the pupils enter their medical course, comprising lectures on anatomy, physiology, chemistry, materia medica, practice of physic, surgery, and midwifery. The only room I did not see was the dissecting room ; it was closed at the time for want of subjects, which it is difficult to procure in a country where so much prejudice against dissection exists, and even against touching a dead body. I was shown into the grand examination room, fitted up with a great throne of state for the Sultan, who presides at the yearly examination of the candidates for the medical degree. There are also a dispensary and hospital attached to the college. The hospital is divided into medical and surgical wards, and a special ward is set apart for diseases of the eye. Dr Spitzer delivers clinical lectures in the hospital, which he kindly invited me to attend.—*Constantinople, March 1848.*

THE PROGRESS OF CHOLERA.

Russia.—The German papers publish letters from St Petersburg of the 3d inst., stating that the cholera has gradually disappeared in that town. The cholera hospitals have been shut up by order of the Emperor.

At the date of the last advices the cholera was fast decreasing, and it was expected that its total disappearance might be soon looked for. The average of cases was reduced to one hundred a day, and several of the hospitals opened exclusively for the treatment of the disease had been already closed.

From a late number of the *Military Medical Gazette* of Russia, it appears that, since the appearance of the epidemic, there were seized at St Petersburg from the 30th of June to the 21st of July, 19,772 persons, of whom 4834 recovered, and 11,068 died. In the whole of Russia, since the first appearance of the cholera, the 28th of October 1846, to the 5th of July 1848, 290,318 persons were seized with the epidemic, and 116,658 died. On the 28th of July there were at St Petersburg 2396 cholera cases ; in the course of the day 137 fresh cases occurred ; 211 recovered, and 82 died. On the 29th there were 2240 sick, 132 new cases ; 188 recovered, and 68 died. On the 30th their remained 2116 cases under treatment. We learn that at Berlin four cases of cholera

have appeared. At Munich the Ministry is taking active preparatory measures in the event of the appearance of the cholera in Bavaria. At Königsberg cases have occurred; in consequence of which a committee of health was sitting in that city to take measures against the spread of the epidemic.

Posen.—The *Kolner Zeitung* has a letter from Posen of the 5th inst., stating that a case of Asiatic cholera occurred in that city. The patient—a woman—was at once transported to the Cholera hospital, where, it is asserted, she is doing well, and likely to recover. Cholera hospitals have, by order of the Government, been prepared in all Prussian towns and large villages, and every care is taken to lessen the horrors of the approaching pestilence.

Vienna.—The *Gazette d'Augsburg* states that several cases of cholera have been observed in this capital.

FRANCE HAS
A COPY

CENSORSHIP IN CHINA.

SOME passages contained in a medical thesis, supposed to reflect on the sacred person of the Chinese Emperor, were condemned by the committee of censorship, and the author was ordered to receive one hundred lashes. The students immediately assembled to the number of 5000, and, with the assistance of the people, disarmed the military. The Emperor, fearing the result, hastily called his council, and revoked the order.

BOOKS RECEIVED.

1. A Course of Lectures on Dental Physiology. By John Tomes, Surgeon-Dentist, &c. London. 8vo. 1848.
2. *Nederlandsch Kruidkundig Archief*, Nitgegeven door W. H. de Vriese, F. Dozy, en J. H. Molkenboer. Leyden. 8vo. 1848.
3. Report of the Directors of the Montrose Lunatic Asylum, Infirmary, and Dispensary, (Instituted 1781), for the year ending 1st June 1848. Montrose. 8vo. 1848.
4. Scriptural Authority for the Mitigation of the Pains of Labour by Chloroform, and other Anæsthetic Agents. By Protheroe Smith, M.D., &c. London. 8vo. 1848.
5. *Littell's Living Age*. With a History of the Ether Discovery. Edited by R. H. Dana, jun. March 1848. Boston. 8vo.
6. Arguments against the Indiscriminate Use of Chloroform in Midwifery. By S. William J. Merriman, M.D., Cantab., &c. London. 8vo. 1848.
7. *Oratio Anniversaria Harveiana*. 1848. A. Francisco Hawkins, M.D., &c. 4to. Londini. 1848.
8. *De Colica Scortorum* Disquisitio, autore Martino Hassing, Dr. Med. Haunizæ, 1848. 8vo. Pp. 100.
9. University Reform, &c., with a Letter to Professor Pillans. By John S. Blackie, Professor of Humanity in Marischal College, Aberdeen. Edinburgh. 1848. Pp. 67.

ERRATA IN AUGUST NUMBER.

P. 103.—In Dr Gairdner's paper, the woodcut was unfortunately inverted. In the course of the volume the two pages will be reprinted correctly; in the mean time, the reader is requested to bear in mind the inversion, and that the explanation of Fig. 19 refers to Fig. 20, and *vice-versa*.

P. 132—line 18 from top, for "Water 3ij" read "Water 3ij".

NOTICES TO CORRESPONDENTS.

DR WATERS' communication on Gunshot Wounds has been received. It was too late for insertion in this Number, but will appear in October.

The "*Bibliothek for Læger*" has been received. Our Numbers shall be sent to Messrs Longman and Co., as directed.

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No. 28. NEW SERIES.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Report on the Early History and Progress of Anæsthetic Midwifery.* By J. Y. SIMPSON, M.D., Professor of Midwifery in the University of Edinburgh.

——— “ I do think you might spare her,
And neither heaven nor man grieve at the mercy.”
Measure for Measure.

MY object in the following Report is, first, to sketch briefly the origin and earlier history of the application of anæsthesia to midwifery; secondly, to adduce some evidence of the present progress and state of the practice, more especially in our own country; and, lastly, to answer some of the supposed difficulties and objections connected with it.

NOTES ON THE FIRST APPLICATIONS OF ARTIFICIAL ANÆSTHESIA
IN MIDWIFERY.

In a communication laid before the Edinburgh Medico-Chirurgical Society in November last, I attempted to prove that the idea of cancelling and abrogating the pains inflicted by the knife of the surgeon had not entirely originated in our own times. I showed that Dioscorides, Pliny, Apuleius, Theodoric, Paré, and others,¹ had long ago described, and some of them apparently practised, the induction of anæsthesia previous to operations, both by giving their patients narcotic substances to swallow and narcotic vapours to inhale. While making the researches upon which the communication alluded to was founded, I further attempted to ascertain if any writer had proposed to assuage or annul, by the same or by other means, the pains attendant upon human parturition. I failed, however, in

¹ Monthly Journal of Medical Science, Vol. 1847-48, p. 451.
NEW SERIES.—NO. XXVIII. OCT. 1848.

finding any traces whatever either of any practical attempts to abrogate or modify, by true anæsthetic means, the pains of labour, or of any theoretical suggestions even as to the very possibility of effecting that desirable result. And I believe the history of the induction of anæsthesia in midwifery does not date far back, like the history of anæsthesia in surgery. The first instance in which the practice was adopted, occurred in my own practice in Edinburgh on the 19th January 1847. The case was one of deformed pelvis, in which I had pre-determined to extract the child by turning, and to try the inhalation of ether vapour upon the mother, with a view to facilitate that operation. During a week or two previously, I had anxiously waited for the supervention of labour in this patient; for, by the result, I expected that much would be decided in regard to the effect of ether-inhalation in parturition. Would it merely avert and abrogate the sufferings of the mother, without interfering with the uterine contractions? Or, would it arrest simultaneously both the contractions of the uterus and the sufferings that arise from them? As far as the proposed mode of delivery by turning was concerned, it was a matter of no vital importance whether the etherization stopped the uterine contractions or not. And, on this circumstance, depended the eligibility of the case for a first trial of ether-inhalation. The result was most satisfactory and most important; for it, at once, afforded me evidence of the one great fact upon which the whole practice of anæsthesia in midwifery is founded—it proved, namely, that though the *physical sufferings* of the parturient patient could be annulled by the employment of ether-inhalation, yet the *muscular contractions* of the uterus were not necessarily interfered with; or, in other words, that the labour might go on in its course, although the sensations of pain usually attendant upon it were, for the time being, altogether abrogated.

This case, with its more obvious results and inferences, was communicated to the Obstetric Society at their meeting on the 20th of January.¹ In the course of the subsequent three weeks I had an opportunity of trying ether-inhalation in several cases of natural labour and in one forceps case; and, at the next meeting of the Obstetric Society on the 10th February, I took an opportunity of bringing the subject under the attention of the members at greater length. In the published reports of the society,² the various inferences which then appeared to me to be deducible in regard to it are given in the following terms:—

1. That the inhalation of ether procured for the patient a more or less perfect immunity from the conscious pain and suffering attendant upon labour.

2. That it did not, however, diminish the strength or regularity of the contractions of the uterus.

¹ Monthly Journal, Vol. 1846-47, p. 639.

² Ibid. p. 795.

3. That, on the other hand, it apparently (more especially when combined with ergot) sometimes increased them in severity and number.

4. That the contraction of the uterus after delivery seemed perfect and healthy when it was administered.

5. That the reflex assistant contractions of the abdominal muscles, &c., were apparently more easily called into action by artificial irritation, and pressure on the vagina, &c., when the patient was in an etherized state.

6. That its employment might not only save the mother from mere pain in the last stage of labour, but might probably save her also, in some degree, from the occurrence and consequences of the nervous shock attendant upon delivery, and thereby reduce the danger and fatality of childbed; and,

7. Its exhibition did not seem to be injurious to the child.

Full details of some of the principal cases upon which these inferences were founded, were, along with other additional instances, subsequently thrown together and published in the form of a communication to the *Monthly Journal of Medical Science*.¹ In that paper I made some observations on the question, Whether it would be proper to employ anæsthesia in natural labour? I adduced various reasons from physiology and pathology for believing, that the parturient action of the uterus would go on healthily and uninterruptedly though the influence of the mind and purely cerebral functions were suspended, and that the dangers of the nervous shock attendant upon labour would be lessened; and I pointed out the necessity of ascertaining, by a cautious series of observations, what counter-indications there might be to the employment of the practice;—whether it were ever apt to give a tendency to hemorrhage or other complications; its influence, if any, upon the child; the length of time its use might be continued in any one case, &c.

At the date at which the paper that I have just referred to was written, viz. the 18th February, the longest time during which I had ventured to keep a parturient patient in the anæsthetic state was about half an hour. And many who believed that this state might be induced without danger for a few minutes, entirely doubted whether it could be sustained for any great length of time without extreme hazard. During the experience, however, of the next two or three weeks, I ascertained the fact, that the anæsthetic action could be safely kept up during labour for one, two, three, or more hours. At one of the subsequent meetings of the Obstetric Society, this result and others were adduced,² and the following additional deductions drawn, as stated in the words of the printed proceedings of the society, viz.—that,

1. The state of etherization had little or no influence upon the foetus, none, at least, of a deleterious kind,—the fetal heart increasing only a few beats, if at all, when the mother was kept long and fully etherized, either during pregnancy or labour.

¹ "Notes on the Employment of the Inhalation of Sulphuric Ether in the Practice of Midwifery."—*Monthly Journal of Medical Science* for March, p. 721.

² *Monthly Journal of Medical Science*, 1847-48, p. 214.

2. The mother, during labour, may be kept etherized, if required, for one, two, three, or more hours. Dr Simpson described two cases, in one of which the mother was about six, and in the other about four hours etherized before the children were born. In both cases the duration of the intervals and of the pains before and during etherization was noted (as in the experiments which Dr Simpson had published on galvanism), and the etherization seemed to have no effect either on their frequency or strength. But,

3. In two or three cases, Dr Simpson had seen a very deep state of etherization modify apparently the full strength of uterine contractions, but they re-occurred immediately in full force when the patient was allowed to fall back into a state of slighter etherization.

4. Dr Simpson had hitherto seen no traceable injury to either mother or child from its employment, but the reverse.

5. The inhaler he used was either a concave sponge saturated interiorly with ether and held over the face, or a simple portable flask without valves.

The first case of labour in which I employed artificial anæsthesia occurred, as I have already stated, on the 19th January 1847. This case and its results were stated publicly, on the following day, to my Class in the University, and immediately became extensively known to the profession through the medium of the public journals.¹ In the course of a short time the practice of anæsthesia began to be tried in other medical schools. On the 13th February, Dr Murphy of London stated to the Westminster Medical Society that he had employed it in a case of turning.² On March 27th a case was published of the use of ether in natural labour,³ by Mr Lloyd. Dr Protheroe Smith delivered a patient under a state of anæsthesia on the 28th of March; and in the *Lancet* for 1st May he published a paper, "On the Employment of Ether by Inhalation in Obstetric Practice," giving an account of this and two later cases in which he had recourse to anæsthesia during labour. He afterwards sent to the same journal several additional cases and remarks.⁴ Mr Lansdowne of the General Hospital, Bristol, subsequently published various cases in which the practice was successfully employed. His first case occurred on the 8th April.⁵

In Ireland, the first case delivered in a state of artificial anæsthesia was on the 28th November 1847. The patient was under the care of Dr Tyler of Dublin. It was an instrumental labour.

In France, the practice was much more early tried. In about a week after the first case occurred in Edinburgh, Fournier Deschamps delivered a patient by the forceps when she was in a state

¹ See Medical Gazette for 1847, Vol. XXXIX. p. 400. Also Provincial Journal for 1847, p. 84.

² *Lancet* for February 27, 1847.—Before this date, viz. on the 26th of January, a state of anæsthesia was attempted to be induced in a patient upon whom the Cæsarean section was performed by Mr Skey of St Bartholomew's Hospital, London. "But the inhalation of the vapour of ether was unsuccessful, or but very partially successful."—(*Lancet*, Vol. I. 1847, p. 140.)

³ Medical Times, 1847, p. 96.

⁴ *Lancet* for 1847, Vol. II. p. 121, and p. 305.

⁵ *Lancet*, Vol. I. 1847, p. 446.

of anæsthesia.¹ This was on the 27th of January. On the 8th February, Professor Paul Dubois exhibited ether in a case of forceps operation at the Hôpital de la Maternité of Paris, and up to the 23d February had used it in four other deliveries. He has not himself published, as far as I am aware, any written observations of his own upon the subject; but he early brought the question, in an interesting and able form, before the French Academy of Medicine,² and various reports of this important oral communication immediately appeared in different journals.³

Professor Dubois' conclusions, as officially reported in the *Bulletin de l'Académie*, were as follows:—⁴

1. The inhalation of ether can annul the pain of obstetrical operations.
2. It can suspend the physiological pains of labour.
3. It destroys neither the uterine contractions, nor the contractions of the abdominal muscles.
4. It diminishes the natural resistance of the perineum.
5. It does not appear to act unfavourably on the health or life of the infant.

Professors Villeneuve⁵ of Marseilles, and Stoltz⁶ of Strasbourg, subsequently published some additional cases and observations.

In Germany the first case of anæsthetic midwifery, of which I have found any published notice, occurred on the 24th February 1847, under the care of Professor Martin of Jena.⁷ The ether was administered during the operation of separating and extracting an adherent placenta. Professor Siebold⁸ read a paper on the subject before the Royal Scientific Association of Goettingen on May 8. He had employed ether in several cases of natural and instrumental labour. On the 3d June, Professor Grenser of Leipsic⁹ etherized a patient for a forceps operation, and afterwards resorted to it in several other natural and instrumental cases.

In America,—the country to which we are indebted for the first knowledge of the anæsthetic effects of sulphuric ether in surgical operations,—the same agent was not employed in midwifery till the reports of its use in obstetric practice in Europe had recrossed the Atlantic. Dr Channing, professor of midwifery in Harvard University, was, I believe, the first to employ it in labour.¹⁰ He adopted the practice in two forceps cases; the first of which occur-

¹ Gazette des Hôpitaux, 30 Janvier 1847.

² Bulletin de l'Acad. Roy. de Médic. Tom. XII. p. 400.

³ "Le 23 Février, M. P. Dubois, qui avait eu connaissance des recherches de M. Simpson, communiqua à l'Académie de Médecine le résultat de ses expériences.—(Chambert. *Des Effets des Ethers*, p. 231.)

⁴ Bulletin, tom. xii. p. 407.

⁵ De l'Éthérisation dans les Accouchements. Marseille 1847.

⁶ Gazette Médicale de Strasbourg, 27 Mars 1847.

⁷ Ueber die Künstliche Anæsthesie bei Geburten. Jena 1848.

⁸ Medical Gazette, Vol. XXXIX. 1847, p. 1052.

⁹ Ueber Aether-einathmungen während der Geburt. Leipzig 1847.

¹⁰ Two cases of the inhalation of ether in instrumental labour. Boston 1847. —(From the Boston Medical Journal.) Dr Channing, in a postscript to an

red on May 5, the other on May 15, 1847. Drs Clark,¹ Putnam,² and other American practitioners, have latterly published the results of their experience in anæsthetic midwifery.

In November 1847, a new impulse was given to the practice of anæsthesia in midwifery, by the introduction of chloroform as a substitute for sulphuric ether. The ether required to be exhibited in large quantities in order to keep up its action ; and hence it was objectionable in ordinary obstetric practice from its bulk, and the inconvenience of its carriage. Most medical men believed that an apparatus of more or less considerable size was necessary for its proper and effectual exhibition, thus further encumbering the practitioner. These and other difficulties were found not to appertain to the use of chloroform, and many, in consequence, adopted it in midwifery, who had previously altogether rejected the employment of sulphuric ether. The first case of labour in which I employed chloroform occurred on the 8th of November. On the 1st December, I reported to the Edinburgh Medico-Chirurgical Society a series of cases illustrative of its effects and use in natural and operative labours.³ In the London weekly journals, since that period, Dr Protheroe Smith, Dr Murphy,⁴ Dr Rigby, Mr Lansdowne, Mr Brown, Dr Bennet, Mr Phillips, and others, have published cases and reports upon the subject.

RESULTS OF THE PRACTICE OF ANÆSTHESIA IN MIDWIFERY.

Since January 1847, up to the present time, I have, in my own practice, delivered about 150 patients under a state of anæsthesia. The results to the children and mothers have been as follows :—

Results to the Children.—In the 150 cases, all the children were born alive except one. In this exceptional instance, the infant was expelled in a decomposed and putrid state, between the seventh and eighth month of utero-gestation. It had not been felt to move, nor had I been able to hear the foetal heart with the stethoscope, for two or three weeks previously. The mother had, before the present pregnancy, borne several premature dead children. Though the infant was small, yet the suffering attending upon its expulsion offered to be excessive, and, to relieve the mother of this unnecessary agony, I placed her under the influence of chloroform.

During the few weeks of my obstetric attendance after delivery

American reprint of one of my Essays on Chloroform, has announced his intention of soon publishing a volume "On the Employment of Etherization in Childbirth."

¹ Philadelphia Medical Examiner, March 1848.

² Boston Medical and Surgical Journal, February 2, 1848.

³ See Lancet for November 20, 1847, p. 533, and December 11, p. 613 ; also Medical Gazette for November 26, p. 934.

⁴ See also Dr Murphy's able Harveian Oration on the employment of Chloroform.

upon these 150 cases, only one of the children died; namely, a child who sank under the symptoms of cyanosis. Nor am I aware that any of them has suffered, up to this time, under "cerebral effusions," or "convulsions," or "hydrocephalus," or any other of the affections which have been prophesied as certain to befall all such infants as would be borne in labours rendered painless by art. Perhaps it may be proper to add, that none of the children have shown any symptoms of what has been calmly averred, in more than one publication in London, as a possible, or rather probable result of anæsthesia, viz. *Idiotcy*.¹—"Dixerit insanum qui te, totidem audiet."

Results to the Mothers.—Among the 150 mothers, the more immediate and direct effect of anæsthesia has been the alleviation or abolition of the physical sufferings attendant upon the latter stages of labour. And, certainly, if the object of the medical practitioner is really twofold, as it has always, till of late, been declared to be, viz. "the alleviation of human suffering and the preservation of human life," then it is our duty as well as our privilege to use all legitimate means to mitigate and remove the physical sufferings of the mother during parturition. The degree of these sufferings is, as a general rule, assuredly such as to call for this aid and mitigation. In proof of their severity, I might cite the unprejudiced testimony of various obstetric authors. At present, I shall content myself with one. Professor Meigs of Philadelphia—a declared opponent of the innovation of artificial anæsthesia in labour—when speaking of the sufferings of the mother in human parturition, fully admits their intensity. "What (says he) do you call the pain of parturition? There is no name for it but *Agony*;"² and he elsewhere speaks of the pains in the last stage "as absolutely indescribable, and comparable

¹ How can we "know or ascertain the possible consequences of the use of such an agent on the brain of the child? And how can we calculate what may be the ultimate consequences of its action in reference to the development of the mental faculties?"—*Dr Malan, in Lancet for April 29, 1848.*

"It is admitted by all, that the pulsations of the foetal heart are *greatly increased* during inhalation,—indeed, to such an extent has this been noticed, that in some instances the pulsations could not be counted, so much were they accelerated. Are not effusions to be feared from this? Are not convulsions after birth likely to ensue? And may not that occur which would make the most heartless mother shudder at the bare possibility of herself, by her want of courage, being instrumental in producing? May not *Idiotcy* supervene? Of this we have as yet no experience, nor shall we have, perhaps, for years; but when *one* such case occurs, will there then be found any one who will afterwards be persuaded to submit herself to etherization during pregnancy?"—*Mr Gream, in London Medical Gazette for 7th September 1848.*—It is perhaps superfluous to add, that the premises of the preceding paragraph are as gratuitous as its conclusions; and that the pulsations of the foetal heart are little, if at all, increased in rapidity when the mother is anæsthetized. "The action of the child's heart (says Professor Siebold) was found to continue quite unaltered, not the slightest change in its frequency and regularity being detected."—*Siebold on the Employment of Etherical Inhalations in Midwifery, in the Medical Gazette for 11th June 1847.*

² Females and their Diseases, p. 49. I leave the italics the same as in the original.

to no other pains." ¹ Now, surely, if it be the duty of the physician (and who doubts it?) to relieve and remove the pains of colic, of pleurodyne, of headache, neuralgia, rheumatism, &c. &c., it is his duty to relieve pains so severe as to be "absolutely indescribable, and comparable to no other pains." There is not one code of humanity for one class of pains and patients, and a different and opposite code for another class of them.

From November last, when I began to use chloroform in labour, up to the present time, none of the patients, with one exception, at whose delivery I have attended, has been aware of these last "absolutely indescribable" pains; the state of artificial anæsthesia having always been induced for a longer or shorter time before their supervention. And I have kept up this state for a period varying from a few minutes to four, five, six, or more hours before delivery. In the exceptional case referred to, the patient's sufferings were greatly mitigated; but the state of anæsthesia was not, as usual, perfect and complete, the patient having been unexpectedly taken in labour when not in her own house, and the attendant anxiety and confusion of herself and her attendants being such as totally to preclude the requisite degree of quietude. When employing ether, I repeatedly saw cases in which the patients were thus only partially and not completely anæsthetized,—where, in other words, they were not entirely asleep, but were aware of the presence of the uterine contractions, and sometimes experienced from them sensations in some degree painful, but of a very mitigated and blunted character.

Besides thus alleviating and abolishing the sufferings of the mother during labour, the practice of anæsthesia carries along with it other advantages. A number of patients have spontaneously told me, that the prospect of being enabled to pass through the ordeal of parturition with the assistance of anæsthetic agents, and without their usual painful agonies, has destroyed, in a great measure, that state of anxiety and dread of anticipation, which, in former pregnancies, had, for weeks and months previously, silently annoyed and haunted them. If we can thus add to the happiness of our patients, by imparting to them feelings of safety and immunity under one of the severest trials to which nature exposes them, we surely follow out, in its truest sense, that which Dr Meigs correctly describes the office of a physician to be,—namely, "a great mission of benevolence and utility."

But the practice of anæsthesia in midwifery not only saves the mother from the endurance of unnecessary mental anxiety and unnecessary physical agony; it saves her also from some of the dangers attendant upon parturition, by husbanding her strength and warding off the effects of that exhaustion and nervous depression which the pains and shock of delivery tend to produce. In most cases the

¹ Philadelphia Practice of Midwifery, p. 153.

mothers, after delivery, on waking from their anæsthetic sleep, have expressed surprise at their own feelings of strength and perfect well-being; and many, who have borne children previously, have gratefully declared to me the great difference which they have found between their condition after being delivered under anæsthetics, and without pain and suffering, and their state of prostration after former labours, when they were subjected to the endurance of all the usual "pangs and agonies" of parturition. Nor does the benefit end here. By annulling the parturient pains and shock, and their direct and primary depressing effects upon the constitution, we ward off, I believe, to a more or less marked extent, the chances and dangers of those secondary vascular excitements which are always apt to follow indirectly upon them. We increase the chances of a more speedy and a more healthy convalescence. And both patients and practitioners have, as a general rule, had occasion to observe, that the period of convalescence has been evidently curtailed and shortened by the previous adoption of anæsthesia during delivery.

Such certainly has been my own experience. For, since following the practice of anæsthesia, my strong conviction is, that I have seen both more rapid recoveries than formerly, and fewer puerperal complications. One patient, however, had a short attack of peritonitis, requiring leeches, &c. It was her third accouchement and her first living child; and, after her two former deliveries, she had required to be bled, and treated for similar inflammatory attacks. At her first labour she suffered severely from puerperal convulsions. In two others of my patients the convalescence was delayed, in one by an attack of the affection described by Dr Marshall Hall as "intestinal irritation" in puerperal females; and in the other by a fit of jaundice, which supervened two or three weeks subsequently to delivery, and after the patient had been for several days in the drawing-room. In December and January last, an epidemic of puerperal fever swept fatally over Edinburgh and other parts of Scotland. During the period of its prevalence, two of my patients were seized with it and died. But the previous employment of anæsthesia in these cases had nothing to do with this distressing result. Some of my professional brethren here and elsewhere, who were not using ether or chloroform, were much more unfortunate than I was. In a district in the neighbourhood of Edinburgh, one of the medical attendants informed me that, at that time, above twenty mothers were attacked and died, and in none of them whom the disease seized upon, did ether or chloroform happen to be used; while several who demanded chloroform during their labours, all fortunately escaped. The first of the two cases which I met with, was after a second labour. The patient's first labour was extremely tedious and prolonged, and, at last, symptoms supervened which demanded the delivery of the child by the forceps. In her second delivery, the labour was much shorter; the second stage lasted only

for about twenty minutes, and during it she was completely anæsthetized. For fifty hours after delivery she progressed most favourably; and, after seeing her at that time with a pulse at eighty, and otherwise well, I was suddenly summoned, in consequence of extremely severe pain having come on in the uterine region after some muscular exertion. Rigors, rapidity of the pulse, tympanitis, &c., supervened, and she speedily sank with all the usual symptoms of puerperal peritonitis. The second case alluded to was in a primipara. The labour was tedious, the pains severe, and the patient was anæsthetized for four or five hours before delivery. For some days after delivery she went on prosperously, until she became unhappily and greatly excited by discovering intemperate habits on the part of the monthly nurse who was taking charge of her infant and herself. A fit of convulsions (a disease to which, in earlier life, she had been long subject) immediately supervened, and recurred several times. Fatal febrile symptoms then set in, with tympanitis and excessive diarrhoea.

I may add that, in the period during which these 150 cases occurred, I have had under my professional charge 20 or 30 other cases of labour in which anæsthesia was not employed, from the rapidity and facility of the delivery, from the patient being too late to send for assistance, from an aversion on the part of patients to the use of anæsthetics, more especially when ether first began to be used during last year, or from other causes.¹ One of the children in these cases was stillborn, and a second died two or three days after delivery. Two of the mothers suffered from crural phlebitis; a third had a severe attack of puerperal fever, but recovered. Two others died; one of them under an attack of puerperal convulsions and coma, which supervened fourteen days after delivery (see details of it in *Monthly Journal* for 1847, p. 213). In the other fatal case, the patient, who had suffered much in her previous labours, came to the immediate neighbourhood of Edinburgh to be confined, and with the view of using chloroform. But the labour proved unusually rapid, and she was delivered before the call for assistance reached my house. Her recovery went on uninterruptedly for two weeks, when a severe attack of dyspnoea supervened. My friend, Professor Miller, her ordinary medical adviser, saw her in my absence, and suspected some acute affection of the heart. When we visited her together shortly afterwards, the symptoms were then apparently those of acute endocarditis. She was submitted to the usual antiphlogistic treatment, and in four or five days felt again so well as to insist with us upon being allowed to rise, which was forbidden. In the course of a few hours afterwards, another fit of dyspnoea suddenly supervened, and, before Mr Miller reached the patient's house, she was dead. We did not procure an autopsy. If

¹ Since November last I have used chloroform in all the cases of labour, where I have been called in time, except two.

unfortunately she had used chloroform during the labour, as was her intention and wish, many of the objectors to its employment would have, I fear, unhesitatingly attributed the fatal issue in this case to its previous employment.¹

In addition to the 150 cases of artificial anæsthesia that have occurred in my own midwifery practice, and to which the preceding remarks apply, I have witnessed, during the last eighteen months, a considerable number of instances in which anæsthetic agents were employed in Consultation and Hospital practice; and I have frequently had recourse to their assistance in various obstetric operations that I have been called upon to perform, as in the separation and removal of the placenta, in various cases of turning, in one cra-

¹ In order to show the caution that is necessary in reasoning upon cases of death, apparently from the exhibition of chloroform during surgical operations, I may add that, since November last, scarcely an operation has been performed in Edinburgh without previous anæsthesia, except where the throat or mouth was the seat of incisions, or the operation itself slight and trivial. Amid all the numerous patients thus operated on in public and private practice, when under the use of chloroform, no kind of misadventure or accident has happened; except one case of temporary fainting, a few minutes after recovery from the state of anæsthesia, be regarded as such. On the other hand, among the few exceptional cases in which, since November last, patients have been operated on in this city without chloroform, two have died on the table. One of the two was being operated on by Professor Miller for a hernia, which had been strangulated for about fifteen hours; when, after the skin merely was divided, the patient complained of great faintness, vomited, and died with the operation unfinished. This occurred on the 8th of November, two or three days after the anæsthetic effects of chloroform were discovered, and nearly proved the first operation in which it was tried. The second case, a patient of Dr Pattison, had an abscess high up in the neck, requiring a simple puncture for its evacuation. He died without hemorrhage, or admission of air, or other apparent cause, a minute or two after the puncture was made. If chloroform had been used in these cases, would it not by some have been blamed for the result?

Twelve or fourteen months ago, Professor Syme was performing primary amputation of the thigh in the hospital, upon a patient upon whom there was no sign of reaction, and who was not etherized for the operation. "Upon the incisions being made, relaxation of the sphincters took place, the contents of the rectum and bladder were voided, and an effort at vomiting seemed the prelude of immediate dissolution. Before tying (says he) the arteries, I waited to ascertain whether the condition of the patient depended upon syncope or death. My colleague, Dr Duncan, by causing alternate pressure and relaxation of the chest, effected artificial respiration for some time without any sign of returning life; but by and by the actions of the system were gradually restored, and maintained through the use of stimulants."—See *Monthly Journal of Med. Sc.*, Vol. 1847-48, p. 76. Such dangerous symptoms, coming on in an anæsthetized patient, might have been mistaken for the effects of the anæsthesia.

Some time ago, before either ether or chloroform was used in surgery, Dr John Argyll Robertson was called, a few miles out of Edinburgh, to perform the operation for strangulated hernia. After having shaved the groin for this purpose, his patient complained of sickness and faintness, and died before any incisions were made.

Last year, Dr Girdwood of Falkirk came to Edinburgh to see the practice of etherization, in order to be able to apply it in a case of amputation. The day for the operation and etherization was fixed; but, some hours previously, sudden apoplexy came on, and the patient died.

niotomy case, and in several patients who required to be delivered instrumentally by the long or short forceps.¹ In all these varieties of operative practice, the previous superinduction of anæsthesia has appeared to me to be of the greatest and most undoubted benefit. For, besides freeing the mother from the additional corporeal suffering and additional mental anxiety attendant upon operative delivery, the state of anæsthesia enables the practitioner to apply any operative interference that may be necessary with more ease and facility to himself, and consequently also with more safety and success to his patient. When the state of anæsthesia is rendered adequately deep, it renders the patient quiet and unresisting during the required operative procedures; it prevents, on her part, those sudden shrinkings and changes of position which the boldest and firmest woman cannot sometimes abstain from when her mind and body have been worn out, as happens in most operative cases, by a previous long and protracted endurance of exhausting but still ineffectual labour pains;—the introduction of the hand into the maternal passages, or of the hand to guide our instruments, is greatly facilitated both by the passiveness and apathetic state of the mother, and by that relaxation of the passages which deep anæsthesia almost always induces; and, lastly, this state of relaxation and dilatibility renders the process of the artificial extraction of the infant through these passages alike more easy for the practitioner, less dangerous for the child, and more safe for the structures of the mother. Besides, in midwifery as in surgery, the utility of anæsthesia before operating, is not, I believe, limited to the mere annulment and abrogation of conscious pain on the part of the patient, and the rendering of the operation itself more easy to the practitioner, but it adds to the safety of our instrumental or artificial interference. For, in modifying and obliterating the condition of conscious pain, the “nervous shock” otherwise liable to be produced by such pain, particularly wherever it is extreme in degree or duration, or intensely waited for and endured, is saved to the already tried and shattered constitution of the mother; and thus, an escape is so far gained from those states of immediate vascular and nervous depression, and of subsequent febrile and inflammatory reactions, that are always apt to follow more directly or indirectly in its train.

MODE OF EXHIBITING CHLOROFORM; DOSE, ETC.

In the course of the preceding observations I have omitted making any remarks on the degree of artificial anæsthesia required in obstetric practice, with the exception of stating that, when instrumental or operative interference is adopted, the anæsthetic state

¹ In one case of placenta prævia to which I was called, the mother had lost much blood, and her lips were pale, and her pulse very weak. On administering chloroform the circulation and pulse rallied—I separated the placenta—no bleeding recurred; and several hours afterwards the child was born. The mother made an excellent recovery.

must be made adequately deep,—so deep, that the patient must be rendered quite passive and apathetic. In fact, when induced for operative purposes in midwifery, the anæsthetic state should be as complete and profound as when it is induced for operative purposes in surgery. But, in common cases of parturition, the anæsthetic agent employed, whether chloroform or ether, does not, in general, require to be given in such large doses as in surgical practice. And in obstetric practice, the rules which I have usually followed in exhibiting the chloroform (the only agent I believe now used in Edinburgh and most other places), are those which I briefly stated when first writing on the subject for this *Journal* in November last. “After the first full dose, a few inhalations, before or with each returning uterine contraction, are generally sufficient. The state of anæsthesia should be made more deep as the head is passing the perineum and vulva.”¹ I have elsewhere in the *Journal* stated these rules at somewhat greater length.—(See No. for April, p. 762).

Occasionally I have at first, and especially in the early stages of labour, given the chloroform in small doses only, so as to obtund or obliterate the sensations of pain, without altogether abrogating the state of consciousness. In many patients, this degree of anæsthesia, with the results stated, viz. the loss, in a great measure, of pain without the entire loss of consciousness, can be readily enough induced, and answers excellently well; but, as a general rule, it has appeared to me in some cases objectionable. For not unfrequently, small doses, such as produce this condition, are accompanied with excitement and talking; and sometimes patients have complained to me of this renewal of the chloroform in small doses with each pain, being accompanied each time with a renewal of the ringing in the ears, flashes of light, and other disagreeable sensations accompanying, in some persons, the primary effects of the inhalation. Besides, we are never thus sure that we are really saving the patient to the full extent by the means we are using. If, on the other hand, she happen to be thrown at once into a deeper state of anæsthesia, the chances of such inconveniences and drawbacks are avoided. Often, when the anæsthetic state is thus made deep from the first, the uterine contractions are arrested for a few minutes, but speedily return. In order to effect this, we take care that as soon as the patient is asleep—(and, in natural labour, we seldom or never require to push the inhalation so far as to affect the respiration, and produce noisy inspiration and snoring as in surgery),—the chloroform should be withdrawn, and not reapplied again till the movements of the patient, or the state of the uterus, as felt through the abdominal walls, indicate a returning uterine contraction. A few inhalations given then, and repeated with each returning uterine contraction, keeps the patient in a state of unconsciousness; and this condition may be easily maintained for hours, by administering in this way the chloroform vapour with each

¹ Monthly Journal, vol. for 1847-8, p. 417.

pain, and withdrawing it entirely during each interval. The practice is not to be expected to come upon medical men by intuition ; for, like all other practices, some care and experience is necessary in order fully to acquire and apply it. And the *two* main difficulties which every beginner meets with are these ; namely, to keep the patient in a state unconscious of pain, and yet not so deeply anæsthetized as to have the uterine action interrupted. For too deep a state of anæsthesia in general interferes with the force and frequency of the uterine contractions ; while a lesser degree of the anæsthetic state leaves these contractions unaffected ; and a still smaller dose often excites and increases them,—the effects, in this respect, of chloroform upon the uterus, being similar to the effects of opium in different doses. But the influence of the inhaled agent passes off in a few minutes, differing in this respect from the more permanent influence of a drug when swallowed ; and if, at any time, the anæsthetic effect is too deep, and the uterine action is in consequence impeded, all that is necessary is to abstain entirely from exhibiting the chloroform for a short time, till the parturient contractions have been allowed to come back to their proper degree of strength and frequency ; and then the anæsthetic agency is to be sustained as before, by giving the vapour with every recurring pain, but in smaller doses, or for a shorter time during each pain, than was previously practised.

Anæsthetic vapours, when given in large doses, have less power of reining up the action of the uterus in the last than in the first part of labour. And as the sensations of pain become more agonizing as the head is distending the perineum, and passing through the vulva, the anæsthetic state usually requires to be then rendered more deep and complete than in the earlier stages of the process ; and in most patients this may be done without at all impeding the rapidity of the delivery. Indeed, in many women, this latter part of the process of parturition seems to be accelerated by the superinduction of anæsthesia ; for the degree of relaxation of the muscular structures of the perineum and vaginal orifice, commonly resulting from it, usually more than compensates for any diminution of uterine action, that may occur. If in any instance it prove otherwise, and the depth of the anæsthetic state interferes too much with the parturient contractions, the simple remedy is that I have already mentioned—a diminution in the state of anæsthesia, so as to allow of a return and increase of the expulsive efforts of the uterus.

The degree and depth of anæsthesia which different patients are capable of bearing without the irritability and contractions of the uterus being impeded, appears to differ greatly in different persons. In some, a very deep state will still leave the uterus almost or altogether unaffected ; in others, its action is interrupted by a comparatively slight degree of the anæsthetic state. It is this variability which at first forms the principal difficulty to those commencing the use of chloroform in obstetric practice. But experience and care will soon enable any attentive observer to overcome this apparent

obstacle, and to adapt the dose of the agent to the powers and capabilities of each different patient. I have never yet seen an instance, but I can conceive it possible, that in some rare exceptional cases and idiosyncrasies, the action of the ether or chloroform should, even in such small doses as merely produce unconsciousness to pain, interfere, especially in the first stage of labour, too much with the muscular action of the uterus, and require to be given up, at least till the labour be more advanced. But this would, of course, be no reason for not employing it in those other persons in whom it had not such an influence; any more than because opium occasionally does not act as an hypnotic on particular patients, it should not be given with that indication to any other patients with the view of inducing sleep.

During the anæsthetic sleep which chloroform induces in natural labour, the patient usually lies perfectly quiet and passive in the intervals between the pains, but moves more or less, and sometimes moans, as each uterine contraction begins to return. In the last stage she generally, with every recurring uterine contraction, makes the usual violent bearing-down muscular efforts, and the struggle can often be marked in the expressions of her face. The muscular action of the uterus and assistant muscles goes on, and yet she remains quite unconscious. The strictest quietude should always be observed and enforced around the patient, for noises and speaking, particularly soon after the chloroform is commenced, will sometimes excite and make her talk; and, if this happen, we may require to exhibit to her a deeper dose than would otherwise be at all necessary. One or two practitioners of midwifery in London have averred and repeated, over and over again, in our medical journals, and in pamphlets intended for non-medical readers, that obstetric patients, under the influence of chloroform, must be liable to talk and act grossly and obscenely. This objection to the practice of anæsthesia in midwifery has been repeated and gloated over by those who have propounded it, in a way forming, apparently unconsciously on their own part, the severest self-inflicted censure upon the sensuality of their own thoughts. An impure mind, more especially in a professional man, may easily fancy and find impurities where none whatever exist; but he is not on that account entitled to imagine, that his own lewd thoughts are typified in the thoughts or actions of his patients. In answer to the supposed objection itself, I have merely to observe, that I never once witnessed any trace of any indecency, either in word or action, in any obstetric patient under the use of chloroform; and the evidence of one and all of my obstetric brethren, at whom I have inquired on the subject here, is to the same effect. In a paper on temporary delirium occurring in the course of labour, Dr Montgomery several years ago described more marked instances of effects of this description, arising merely from "the extreme distress and pain" to which the mother was subjected in the dilatation of the os uteri, &c., during natural parturition, than were ever seen to arise from the influence of means used to abate and abrogate that "extreme distress and pain."

In administering chloroform in obstetric practice, I have always used the handkerchief, as the simplest and best apparatus. Sometimes, when the case is likely to be tedious, I have it folded and sewed into the form of a deep cup or cone. The chloroform is poured into the bottom of the cup, the open end of it held over the nose and mouth of the patient when the action of the vapour is required; and, when its application is suspended, by closing the open end of the cone, the escape and loss of the vapour is prevented during the intervals. Such an arrangement saves the chloroform. But a handkerchief merely folded together and sprinkled with chloroform, answers quite well; and in the intervals it may be compressed together in the hand, so as to prevent the escape of the chloroform. In *first* throwing the patient over into the anæsthetic sleep—(the point which requires the most management),—a handkerchief thus presenting a large surface is often much more serviceable than one folded into a cup shape; for the patient, when first coming under the influence of the chloroform, is apt to move her head from side to side; and, in order to keep up the constant inspiration of the vapour, she can be more easily followed by using a simple handkerchief, than by trying to keep any kind of apparatus applied to her mouth or face.

The quantity of chloroform used varies both according to the duration of the labour, and the susceptibility of the patient. Usually, when the handkerchief is used, about an ounce an hour is necessary, a small quantity being poured upon it from time to time. A less dose will suffice in some, and others require more. In one case lately, where the patient, in a first labour, was anæsthetized for two hours, I expended nearly six ounces, large doses being necessary to keep her in a sufficiently deep state of unconsciousness. The first quantity which I pour on, usually amounts to three or four drachms; but I always judge by the *effects*, not by measuring the dose; and I pour on an additional quantity in a minute or so, if it be required. In holding the handkerchief towards the patient, I take care that plenty of atmospheric air is admitted,—and seldom or never put it in contact with the face. At first, it is better to hold it at a considerable distance, in order to prevent any chance of irritation and coughing; and then gradually approach it. It is always to be remembered, that the vapour of chloroform is nearly four times the specific gravity of atmospheric air; and if the patient is lying on her side, the handkerchief or pillow can be easily arranged so as to keep a larger supply of this heavy vapour opposite the mouth and nostrils. I have always held and managed the handkerchief myself in the first instance, and till the patient was asleep. Afterwards, I have generally trusted it to the husband or nurse, teaching them to apply it near the face when the pains supervened; and folding up the handkerchief in the way mentioned, so as to preserve the chloroform during the intervals.

When exhibiting chloroform in obstetric practice, and in the way I have described, I have been often struck by the circumstance, that

its use is very rarely followed by sickness or vomiting. I do not remember having seen vomiting follow its exhibition in labour in more than four or five cases, and two of these (in the practice of Dr Paterson and Dr Cochrane), were instances in which I was called in to apply the forceps, and where the patients were placed for the operation in a state as deep as that used in surgery. I have repeatedly seen it arrest the sickness and vomiting occasionally accompanying the first stage of labour.

In addition, let me state, that I have usually begun the employment of the chloroform when the os uteri was well dilated, or towards the termination of the first and the commencement of the second stage of the labour. But when the pains were severe I have commenced it earlier, and when the os uteri was still comparatively little dilated. There is, I believe, no limit as to the date of the labour at which we may give it.

REPORT OF THE RESULTS OF ANÆSTHESIA FROM DIFFERENT OBSTETRIC HOSPITALS AND PRACTITIONERS.

The following account of the results of anæsthesia in the practice of the Maternity Hospital, Edinburgh, has been drawn up by Dr Duncan and Mr Norris, two gentlemen who have acted as resident house-surgeons in the institution, and upon whose power and accuracy of observation all who are acquainted with them will place implicit reliance.

Since the use of anæsthesia in labour became general in the Maternity Hospital, shortly after the discovery of chloroform, 95 women in all have been delivered in the house under its influence. Among these, 88 were natural and 7 were morbid labours. In the 88 cases of natural delivery, only one of the mothers died, convulsions coming on five hours after delivery, and proving fatal after a continuance of six days. On a *post mortem* examination, the kidneys were found to have undergone, in some parts, the true steatoid degeneration.—(See a report of the case, *Monthly Journal for September*, p. 196.) Among the same 88 cases of natural labour, there were 5 dead-born children. In 2 of these cases, the birth was premature, being at the sixth month. In the third case, the mother had previously given birth to two dead infants. The fourth dead child had a very large hydrocephalic head. The proportion of still-born children was thus 1 in 17. In the Dublin Hospital the proportion of still-born children, as reported some years ago by Dr Collins, was 1 in 15. The 7 instrumental cases were as follows;—1 application of the short forceps, in an unsuccessful attempt to save the life of the child; the mother recovered well;—2 cases in which the long forceps were applied; one of the women, in whom the head was very long impacted in the pelvic brim, died from sloughing of the maternal passages; the other made a good recovery; both the children were born alive;—4 cases of version; one of the mothers died from rupture of the uterus, the others recovered quickly; 3 of the children were still-born, and in one of these 3 cases the cord was prolapsed.

In addition to these eighty-eight cases of anæsthetic delivery, there have been upwards of fifty women delivered in the house without chloroform. These have been chiefly very rapid labours, where the women have come into the hospital just in time to give birth to their infants, or where the house-surgeon has not been able to see them till very shortly before delivery. From

the expense attending a large consumption of chloroform, it has always been an object to husband it as much as possible;¹ and therefore, in the hospital, it has not been given in cases where the mothers did not very severely complain of their sufferings, nor were harassed with feelings of anxiety and fear.

On the whole, the results of anæsthetic midwifery, as observed by us in the Hospital, have been perfectly satisfactory; and we can confidently state that the recoveries have been altogether more perfect and speedy than before. This has been remarked in so great a proportion of the cases, that there can be no doubt whatever of the truth of the observation. Besides the increased rapidity of recovery, we have noticed the almost entire absence of those uncomfortable feelings of fatigue, languor, and shivering, and of that shattered feeling which so frequently comes upon the mother immediately after an ordinary delivery. Instead of this, we have found the mother almost invariably awake from the anæsthetic sleep comparatively fresh, easy, and cheerful. Not unfrequently the anæsthetic has been found to change, without an intermission, into a natural sleep, which may continue for an hour or two.

Further, there have been, since the introduction of chloroform into the practice of the hospital, far fewer than formerly of those violent attacks of rigors, ephemeral fevers or weeds, and abdominal pains, which are so common in most crowded hospitals, forming a class of cases which used formerly to cause much anxiety, and was a common cause of the mother's being detained in the hospital after the usual fortnight allowed for recovery. In fact, since using chloroform, there have been scarcely any women detained in the house by these causes, and much less Dover's powder, calomel and opium, abdominal fomentations, &c., have been used.

The women have been, invariably, found deeply grateful for the relief to their sufferings afforded by the anæsthetic influence of chloroform.—Yours, &c.

J. M. DUNCAN. H. NORRIS.

At a meeting of the Edinburgh Medico-Chirurgical Society in June last, along with other practitioners, I gave in a report on the employment of chloroform in midwifery. At that time, and since, I have been favoured with written statements of the results by various medical friends in Edinburgh, and in different parts of the country. I shall now give extracts from a variety of the letters which I have received relative to this subject. Many more such communications might easily have been called up and adduced; but I have deemed it useless to multiply unnecessarily this kind of evidence. It will be observed, that, with one exception (see the communication of Mr Lansdowne), the following letters refer—like the preceding statements regarding the Maternity Hospital—to the use of chloroform alone.

The first statement which I give is from my assistant and friend, Dr Keith.

I have employed chloroform in every case of labour under my care, since its introduction, with one exception; and also in almost every case to which I have been called in by other practitioners. In my own cases, amounting to about four-and-twenty, it has been given for a period varying from half an

¹ Perhaps, in a short time, a benevolent government will allow chloroform to be made cheaper, by removing the very high duty on proof spirit when used for medical and chemical purposes (tinctures, &c.) At present that high duty is, in one respect, a direct tax upon the relief of human disease and the mitigation of human pain; and a great obstacle to the progress of British organic chemistry.

hour to eight hours. The quantity of chloroform consumed has been, on an average, about one ounce per hour; in a few cases double this quantity was found requisite. The anæsthesia has been in almost every case complete; that is, the patient, on awakening, has declared, that while under the influence of the chloroform, she was utterly unconscious of all pain. In most cases the patient has lain quiet even during the pains, the presence of which is then generally indicated by the breathing becoming more rapid and somewhat laboured. In other cases there is suppressed moaning during the pains, or even, in some, loud manifestations of powerful straining and muscular exertion. I can state most positively that I have seen no serious symptom which could be traced to the chloroform, in any one case, either as affecting the mother or the child. Most of the mothers have made uncommonly good recoveries. Those who have had children previously, have, almost without exception, stated to me, that they felt very decidedly stronger after delivery than on former occasions. In two cases the recovery was rather slow, but this was owing to the patients' having been in a very delicate state during pregnancy,—and, in both instances, I considered the chloroform was of very great service, by saving their strength. All the mothers are now in their usual health.

In no one of the twenty-four cases was the child still-born. In one case labour was brought on at the end of the seventh month, owing to the brim of the pelvis being much contracted. The child was born alive, but died on the second day. All the other children are now alive. They have all been nursed by their own mothers, with one exception.

I have had occasion to use the forceps seven times since the introduction of chloroform, and once to break up the child's head and extract by the crotchet. In all these cases the patient was first put into a deep anæsthetic state, and in most she lay perfectly still and apathetic during the operation. All the mothers have done well, except in the case of craniotomy, where the uterus had ruptured previous to the use of chloroform.—Yours, &c. G. S. KERR.

From Dr Moir, Edinburgh.

Since the beginning of December I have, with a very few exceptions, used chloroform in the course of my midwifery practice, and I have not met with a single case where any unpleasant effects, either to mother or child, can be traced to its use.

As far as my observation has gone, I think it will be found that, in some cases, the chloroform, if freely administered at an early period of the first stage, retards the pains a little, and in others also lessens their power; and when this does occur, the best remedy is either to intermit its use till the labour is further advanced, or to give it in smaller quantities and at longer intervals, so as not, at that stage, to induce complete unconsciousness. But, whether correct or not in this opinion, I am quite satisfied that the second stage is much accelerated, especially towards its termination, by the chloroform doing away with the resistance offered to the expulsion of the head by the muscles at the outlet of the pelvis,—and this to such an extent, that, in some *first* cases, there is a risk, unless very great care is taken, that the perineum be slightly lacerated, from the head being so rapidly expelled as not to give time to the parts to yield so rapidly as they would otherwise do. But this is comparatively a very rare occurrence, and requires to be mentioned principally with the view of putting young practitioners on their guard against it, and of leading them to use the necessary means to prevent it.

In exhibiting so powerful an agent as chloroform, I think it a point of importance to use as small a quantity as is compatible with the obtaining of its full anæsthetic effects; and, as this seems to depend much upon the rapidity with which it is conveyed into the system, it seems a point worthy of consideration to ascertain the readiest means of so doing. Various instruments have been invented for this purpose, though they have been almost universally superseded by the use of the handkerchief, as recommended by you, and used either in the form

of a hollow cone, applied again and again, after renewing the chloroform, over the nose and mouth of the patient; or simply folded up several times, and frequently having interposed between the folds a piece of wool or flannel, with the view of better retaining the chloroform. In both methods there is a considerable loss of chloroform, much of it being retained in the folds of the handkerchief, and much of it escaping without passing into the lungs, or else passing into them so slowly as not to produce the desired effect. I have, for some time, been in the habit of using a linen or white cotton handkerchief, folded only once, or, if very thin, folded twice; the point requiring attention being, that it should not be so thick as to offer any impediment whatever to free respiration when applied over the mouth and nostrils. Since using the chloroform in this way, I have never failed in rapidly producing the anæsthesia, either in my own practice, or when accompanying some of my patients (who were several months advanced in pregnancy) to their dentists to have one or more teeth extracted. For administering the vapour to patients who are in the erect position, the chloroform should be poured on that part of the handkerchief placed on the palm of the operator; the edge of the little finger should then be applied close to the chin, and the hand gradually raised up towards the mouth, till the sensation of choking which generally accompanies the first inspirations has passed off, after which, the handkerchief should be left on the face and the hand removed; the patient then breathes freely through that part of the handkerchief wetted with the chloroform, and, in general, half a drachm is sufficient to produce anæsthesia.

In obstetric practice, I find it the most convenient plan to place one end of the folded handkerchief under the left cheek of the patient, to pour a little chloroform on it, and then, taking hold of the loose end of the handkerchief, to bring it gradually near the mouth, till it can be left there without inconvenience, the patient breathing freely through it. And it is not necessary again to remove the handkerchief, but simply to pour on it occasionally a very few drops, whenever the patient begins to show symptoms of returning consciousness, or on the accession of a pain. The only precautions necessary are, to raise a small fold of the handkerchief from the skin when the chloroform is to be applied, so as not to blister the skin; and to drop the chloroform, not on that part of the handkerchief immediately over the mouth, but a little above it, so that the vapour, being heavy, may flow down towards the mouth or nostrils, and thus be, during inspiration, more readily received into the lungs. By adopting this plan, I have had the handkerchief applied for nearly two hours without removing it; and the quantity of chloroform I have used in single patients, has been much less than other practitioners have used in cases of the same duration.—Yours, &c.

JOHN MOIR.

From Dr Malcolm, Edinburgh.

Since November last I have employed chloroform in above thirty cases of labour, and with the most satisfactory and delightful results. A majority of these were first labours. I have kept my patients under it for periods varying from half an hour to six hours, and have never found the slightest unpleasant effects result from its use. All the children have been born alive, and are at this moment in perfect health, with the exception of one that died when about a month old, of a sudden and severe attack of dysentery. All the mothers have made recoveries with rapidity and completeness, far above the average which I had previously observed in my practice. This has struck me as the more remarkable, seeing a large proportion of my patients were primiparous; and I can only attribute this result to the entire absence of suffering and shock to the nervous system which is effected by the use of chloroform. Although in a few cases my patients and their friends have at first objected to the use of anæsthesia to abolish pains which they considered "natural," yet every one has afterwards expressed to me sincere gratitude for saving them from their agonies; and I am sure not one who has experienced the beneficial effects of the practice will ever submit to these agonies again, now that they know that they are so totally unnecessary, and can be so easily and safely abolished. I have repeatedly found the mothers

of my patients object to anæsthesia, as if they grudged that their daughters should not experience the same sufferings as themselves,—but I have uniformly found them afterwards as grateful as their daughters for the relief administered.

Generally, I have employed about an ounce of chloroform per hour. I have never seen the uterine contractions arrested by its use, although I have no doubt a large dose would, when necessary, have that effect. I have seen no case of hemorrhage, or convulsions, or any other complication whatever. Let me add, that I cannot conceive on what principle the employment of chloroform in natural labour should be objected to, as long as it is our duty, and assuredly it is our duty, as physicians, to relieve and mitigate human suffering.—Yours, &c.

R. B. MALCOLM.

From Dr Thomson, Edinburgh.

After stating the details of ten cases, Dr T. remarks :—

Among the middling classes I have met with more difficulty in using chloroform than I had anticipated, as it has only been when the sufferings of the patient were very severe, or her friends had begun to dread the effects of prolonged continuous suffering on her constitution, that I could get my wishes carried into effect. That fatal Newcastle case, which was trumpeted a good deal in the newspapers, is still haunting their minds, and is very frequently urged by some timid friend when you propose the chloroform to relieve the sufferings of the patient.

My experience of it has been in all obstetric instances analogous to your own; with one exception, I have had no difficulty in getting the patient under its influence: a minute or two was in general sufficient to lay the most restless or ungovernable patient quiet on her pillow.

Not the slightest *post-partum* hemorrhage has taken place in my ten cases, though in the two forceps cases, where it was given deeply, the uterus remained flabby for nearly thirty or forty minutes, and threw off the placenta with difficulty.

Its relaxing effects are, I think, undeniable. In one case, the soft parts had resisted for a considerable time the descent of the head; they yielded very readily within an hour after the chloroform was begun. I have not had another instance of this kind lately; but, were I to meet with one, I feel confident it would yield with much more facility under the chloroform than without it.—Yours, &c.

ALEX. THOMSON.

From Mr Carmichael, Edinburgh.

I have given the chloroform in twenty-six cases of midwifery, four of which were first labours; the others varied from the second to the eleventh pregnancy. The quantity given varied from two drachms to four oz., and the length of time during which it was exhibited from a few minutes to four hours. The preparation I have used has always been that of Messrs Duncan, Flockhart, and Co., and I have never seen the slightest bad effects from it, either in midwifery or other medical cases, or in any case where I have administered it for amusement, except occasional sickness where it was exhibited shortly after a meal.

I have met with no case of flooding whatever. I have heard it alleged that it drives away the milk; but I have not found it so, as my patients have all been able to nurse, with the exception of one lady, who has not been able to suckle her child for the last three times.

In all of these cases it was administered with the greatest ease and with perfect success, and, in no case, with any bad results.

The recoveries have been certainly more than usually speedy. Indeed the only objection I have met with as to its use, has been on the part of the monthly nurses, who seem afraid that the new practice will curtail their attendance and pay.

All the children were born alive, and are doing well. The only case in which the child proved the least refractory was a footling one.

I have also found it most useful in cases of dysmenorrhœa, in spasmodic colic, and tic douloureux.

I also gave it, in a case which you saw along with me, of most severe neuralgia of the uterus, with the most perfect success.

In no case whatever have I seen any bad results of any kind arise from the use of the chloroform.—Yours, &c. W. S. CARMICHAEL.

From Dr Burn, Edinburgh.

I regret that I cannot give you the number of cases of labour in which I have exhibited the chloroform, but I may state that I have given it repeatedly, and have not seen any bad consequences either to the mother or child result from its use.

All the mothers made rapid recoveries, and the children did not appear to suffer from its use.

I have given the chloroform in three or four cases of adherent placenta where the uterus was firmly contracted, and had far less difficulty in extracting it than I have experienced in similar cases where the chloroform was not exhibited.—Yours, &c., J. BURN.

From Dr Purdie, Edinburgh.

I have now used chloroform in seventeen cases, which I have noted, and in every instance with decided effect, not merely by lessening suffering, but I am perfectly convinced, by the most careful observation, by shortening the duration of labour. The pains have never in my experience been interfered with, except by rendering them quicker, and far more effectual.

There is one of the cases which I would wish to recall to your memory. The patient, thirty-eight years of age, was in her first labour, which commenced early on Wednesday morning, and went on well but slowly till the evening, when its progress ceased, although the pains continued regular and strong. The os uteri was well dilated, but the head made no progress, although there was no very evident cause. About one o'clock on Thursday morning I sent for you to deliver her with forceps. On your arrival, you thought that still there was hope of the labour being terminated naturally. The patient, who was suffering much, was then put under the influence of chloroform and ergot, while you waited patiently for any advancement, for nearly two hours, without effect. You then delivered with the long forceps, which cost you great exertion, from the head being impacted in the brim. The patient's position was changed, the placenta was extracted, she was bound up and laid in a comfortable and easy posture, in which state she continued to sleep soundly until she was awakened after the child was dressed, the crying of which surprised her, as she had not been conscious of what had taken place from the time she got the first dose of the chloroform after your arrival. This patient had an excellent recovery. I never saw a patient suffer less after labour, or recover more rapidly. I may just add, that there are few things vex me more with regard to patients, than to witness the sufferings of a childbed patient, who will not allow, from ignorance or prejudice, the use of chloroform. Happily, however, such cases are very rare among us.—Yours, &c. W. PURDIE.

From Dr Finlay, Newhaven, near Edinburgh.

I have used chloroform in a considerable number of cases of natural labour. It was with much reluctance that I first administered it, and only at the urgent entreaty of a patient who was enduring intense agony before the birth of a first child. It was completely successful. Her screams had been audible across the street. In a few minutes they ceased, and she fell asleep, while the uterus continued to act as powerfully as before. She was not aware that she had got her baby until a quarter of an hour after it was born. In five of the other cases the

influence of the chloroform was as complete. In the other three cases *entire* insensibility was not induced, but the sufferings were greatly mitigated, and the relief was so evident to the patients, that, whenever the labour pain was approaching, they grasped with great eagerness the handkerchief on which the drug was sprinkled. Satisfactory as these cases have been, I have hitherto used the chloroform with considerable hesitation and caution, and only when it was asked for by the patient. But every trial has emboldened me to employ it with greater confidence on future occasions. In each of my cases the placenta was soon and easily detached; in none of them did hemorrhage occur; and they all made excellent recoveries.—Yours, &c.

A. FINLAY.

From Dr Cumming, Edinburgh.

I have now attended thirty-five cases of labour under chloroform, and it has been used in all with marked advantage. All the patients have made unusually good recoveries; and I have been very much impressed by the fact, which was remarked by the first patient submitted by me to chloroform, and repeated by all, that the convalescence was not accompanied by the crushed and dislocated feeling that they have experienced without it.

I always begin by introducing the chloroform slowly and gradually into the lungs, allowing a large proportion of air to be inhaled along with it. In every instance it was administered, not with reference to the quantity given, but to the effect produced,—this effect being complete unconsciousness during the pain; and thus administered, I have never seen any unpleasant or absurd consequences, nor any thing to excite alarm or even uneasiness.

Two of the cases had had large floodings in a previous labour; with the chloroform there was none. This, of course, I do not impute to the medicine; but it at least tends to prove, that hemorrhage is neither a necessary nor a likely consequence, as many at first were disposed to imagine.

All the children were born alive, and are so still. None of them as yet give the slightest indications of idiocy, either present or future; nor have I observed in any the temporary stupefaction immediately after birth, ascribed to the presence of chloroform in the apartment, that some have remarked.

I am quite satisfied that, if properly given, it acts as a calmate; and I believe, from what has passed under my observation, that very many of what are called exceptional cases are not so in reality, but appear to be such from error in the mode of administration, and that further experience will amply demonstrate the truth of this.

In short, I am, unfortunately for the appearance of veracity, compelled to say, that all my cases hitherto have been so successful, the recoveries so uniformly good, and the satisfaction on the part of the patient (I may add also my own) so great, that I am rapidly approaching to, if indeed I have not already arrived at, the conviction, that, *if there be any sin connected with chloroform, it is chargeable on those who refuse to administer it.*

I may add, that not one of those patients who have already inhaled it will ever be denied it in any subsequent pregnancy, as they have repeatedly assured me; and certainly I shall not attempt to keep it from them, and that not more for their sake than my own.—Yours, &c.

W. CUMMING.

If necessary, I might have adduced more evidence in favour of the anæsthetic effects of chloroform in midwifery practice, from Dr Beilby, Dr Ziegler, Dr Weir, Dr Young, Dr Menzies, Dr Gilchrist, Dr Campbell, and other medical practitioners in Edinburgh who have been using it.

In order to vary the kind of evidence, I shall next adduce extracts from various communications which I have received on the

use of chloroform in midwifery, from medical correspondents in different parts of Scotland, England, and Ireland. It is needless, I believe, to attempt to arrange them in any special order; and I shall content myself, therefore, with beginning with the letters of those practitioners who live farthest north, and proceed southward.

The following is an extract from a letter written to me this summer :—

From Dr Grigor of Nairn.

Dr Allan of Forres and myself would as soon think of going to an obstetric case without our chloroform phial, as we would of going to bleed a patient without a lancet. In this quarter, doctors are only called in when things are going wrong, or in extreme cases; so that, since your grand discovery, he and I have only used it in about twenty-four cases, in all which it came up to all you have written about it,—no still-born children—mothers recovering well—fewer after-pains, &c. &c. One of my cases was a first child, the mother nearly forty-eight years of age, weakly in constitution, and of small formation. Had it not been for the chloroform, I do think she would have sunk.—Yours, &c.

J. GRIGOR.

Dr Dyce, lecturer on midwifery in Marischal College, Aberdeen, favoured me some time ago with the following interesting communication, regarding the obstetric employment of chloroform :—

I have reports from my friends Drs Harvey, Pirrie, and Gilchrist, all of whom, I was aware, had been employing it. I may at once state that I consider it a most invaluable agent; that I have every confidence in its safety; that I recommend it almost on every occasion; that no evil consequences have ever attended its use; and that I have found its effects nearly alike in all. I have used it eleven times. The labours have, with one exception, been natural. The exception was a breech case, and a first child. Two of the eleven children were lost; one was putrid, and in the other (the breech case), though the funis did pulsate for some minutes, the child could not be recovered.

Dr Harvey has given me a brief account of four cases delivered under chloroform; all the children were born alive.

Dr Gilchrist at Woodside writes me, "I have observed no evil results to the infants themselves from the use of chloroform." He does not state the number of his cases; but I presume they have been numerous from his remarking, that "latterly I have not used this agent so generally as when it was novel, reserving it now, unless when urgently requested by the patient, for cases unusually painful, whether arising from excessive sensibility of the system, rigidity of the soft structures, or cases requiring manual assistance."

Dr Pirrie has employed chloroform in fourteen cases. Ten of these were natural, two instrumental, and one a case of turning. He says, "as to the children, they have all been born alive, and continue to go on satisfactorily."

I never use it early in labour; generally the second stage has come on, or at least the os uteri is tolerably well dilated. This appears to be the practice of my brethren here. I then keep the patient in a state of insensibility more or less complete during the future progress of the labour. Occasionally I have allowed them to come completely out of the anæsthetic state; but, on the recurrence of the pain, the patients invariably and urgently seek for the handkerchief. When I used chloroform, I imagined that the intenseness of the stupor induced, did lessen the frequency and force of the pains, and even put a stop to the labour altogether; and I am still of opinion, that if the full effect is produced and kept up for any time, the pains will cease; but if a more moderate

effect is produced, so that consciousness to a certain extent remains, my conviction is, that instead of checking uterine action, its use enables the organ to act with more freedom; the mental influences are quickened; and, more than this, it has a decided power in relaxing the soft parts, and thus removing a very frequent cause of protracted labour. I had a very interesting case illustrative of these remarks in January last, in a lady in her fifth pregnancy, of a very irritable, anxious, and highly nervous temperament, who had previously to labour determined to use this wonderful agent. Her former labours had always been tedious, and very painful throughout every stage. This commenced and had continued for a couple of hours before I saw her. She was complaining much of the severity of the pains, especially of her inability to move from one posture. Her anxiety and agitation were very considerable; her pulse was above 100, and her body was already wet with perspiration. On examination I found the parts rigid, and the os not larger than a shilling, while the rectum actually encroached upon the vagina from its loaded state. I determined, therefore, on emptying the gut by an enema before employing the chloroform, and mentioned this to my patient, and gave the nurse the necessary instructions. I very soon found that I had added not a little to her already excited state, as she was not prepared for delay, and would only, after some persuasion, consent to its exhibition, and only then on condition that immediately after she was to be allowed the "stuff." Her request was complied with, and I confess to you its effect on her general state from the moment she was under its influence, not only gratified myself, but pleased her friends, who complained much of her impatience and imagined evils. A few inhalations sufficed to induce insensibility, before which she gave one or two hysterical laughs, and made some incoherent remark. She then became still and quiet, until a pain returned, —the only indication of which was a gentle writhing of the body. Her pulse very soon fell to 80, at which it remained; her skin became cool; the parts rapidly relaxed; she moved, and allowed herself to be moved in any direction readily: the pains were certainly more regular and efficient than in any of her former labours; and in two hours and a half from the first inhalation, the child was born. Once only during this period, and that was after one hour had elapsed, was she perfectly sensible. This was permitted at the solicitation of the nurse and friends, who had not seen chloroform used before, and could not be convinced that so sudden a change could exist and the labour progress perfectly. They soon had evidence of this, by the patient screaming out in her former impatient tone, "Pain, pain! where is the handkerchief!" From this time until delivery she was perfectly unconscious. The placenta came away naturally in ten minutes. The roller was put on, some of the soiled things were changed, and it was not for several minutes ere she was convinced that her trials were over. I need hardly say that she was gratified in the highest degree to find that she had escaped so much of her former suffering, and expressed her deep thankfulness for so valuable a boon. The child was putrid. Her recovery was perfect, and more rapid than on any former occasion.

In my practice I have met with no instance of chloroform failing in producing its usual effects in a very few minutes, where it was willingly and readily inhaled; in some nervous patients, a few seconds are sufficient for this purpose. In the case I have just given, I was surprised at the few inhalations she took; and Dr Pirrie mentions that one of his patients was so highly susceptible of its effects, that, on making three or four inhalations, the state of anæsthesia became so complete, that the labour was entirely suspended; he therefore, in this case, discontinued its use. On some occasions I had difficulty in persuading the patients to use it; hence a longer time elapsed. In one case when persuasion failed, I watched an interval of drowsiness, and in spite of herself brought the woman fairly under its influence, in which state she remained for twenty minutes, when the child was born unconsciously. Its cry

aroused her; she turned herself hastily around with an enquiring look, and after a momentary gaze said, "What is that? you have been giving me the stuff."

The quantity of liquid used has varied in my hands from $\frac{3}{4}$ ss to $\frac{3}{4}$ iiss. This last quantity was expended in the case of a young woman, of eighteen years of age, pregnant of her first child, and who was more or less under the influence of chloroform for five hours. As this was amongst my first cases, if not the very first, and the longest period in which I have employed this agent, perhaps you will excuse my giving it somewhat in detail, more especially as ergot was given along with it, which I am not aware that I had seen any where recommended. Jane —, æt. eighteen, unmarried; first child, December 18, 1847. When seen at five p.m., the membranes were reported to have ruptured twelve or thirteen hours before; on examination, the os uteri was found dilated to the size of half-a-crown, and the breech was discovered as the presenting part; the pains were frequent, irregular, at times only effectual, but they had been increasingly severe during the after part of the day, and now were strong.

About seven p.m., after some persuasion, she first inhaled the chloroform. She soon became insensible; it was then only presented to her at intervals as a pain recurred, a single inhalation being generally sufficient. She laughed and sang for a few minutes, but at length seemed perfectly unconscious, and we were only sensible that she had pain by her now and then rolling about, as is the case when labour-pains continue under convulsions. For an hour she was kept in this state; but thinking the pains were becoming more distant, and the progress of the labour slower, she was allowed to come out of this torpid state. Soon after $\frac{3}{4}$ ss. of powdered ergot was given, and repeated four times at intervals of a quarter of an hour. The pains now became stronger and more frequent, and my patient, who was conscious of the powerful agency of the chloroform, and the advantages she had derived, insisted on its repetition. She had, in fact, secreted the handkerchief, and was now keeping it at her mouth. But, alas! its talismanic powers had vanished; she threw it rather angrily at the student, who, at that moment, sat by her bed-side, desiring him to give her the bottle in its stead; she assured us that she had felt no pain until now, that she had been dreaming of her illness, which she believed was finished. From this time (nine o'clock) until her delivery, which took place between one and two o'clock the following morning, she was kept in a perfectly unconscious state, at times more so than at others, according to the severity of the pains. Towards the end of the labour the throes were very severe, but during none of them did the patient complain of the slightest pain. The only indication, as I have already said, of apparent suffering, was a movement of the body, and occasionally a low moan. The child in this case did not breathe or cry; it was not therefore legally in life, though the funis pulsated for some minutes.

I can scarcely think that the death ought to be attributed to the chloroform, or even to the ergot. The length of the labour, the nature of the presentation, and especially its being a first child, were almost against its being born alive. So that, with this exception, no injury has befallen the infant in the practice of any of us here who have employed chloroform.

I have only further to say, that the recoveries in every case have been most satisfactory. They appeared even more rapid than under ordinary circumstances.—Yours, &c.

R. DYCE.

From Mr Lawrence, Montrose.

I am sorry I cannot furnish much information as to the employment of chloroform in midwifery in this quarter. It has been very little used by myself or brethren, owing chiefly to a very general prejudice on the part of our parturient patients against it. The very last case I attended, the patient resolutely refused it, although suffering very severe pain! However, in one of those in which I administered it, and to which I had gone unprovided with the chloroform, my patient compelled me to send for it! In one case in which

a patient of mine had convulsions in her first labour, I exhibited chloroform at her second confinement with the usual success.

I have seen no injurious consequences of any kind.—Yours, &c.

SAMUEL LAWRENCE.

From Dr Steele, Montrose.

I have used chloroform in midwifery practice on six separate occasions. In two of the cases, it was given to the extent of producing only a partial immunity from suffering; but with the effect of converting, especially in one to whom it was administered for about four hours, what would have been a very severe, into a very easy labour. In the other four the effect was highly satisfactory; two of the children being born without the consciousness on the part of the mothers, for some time after, of that event having taken place. One lady remarked, that she thought "the march of intellect had never taken a happier direction than when it led to the discovery of chloroform."

All the children did well. One of them, however, was asphyxiated for nearly a quarter of an hour after its birth.

From the little experience I have had of chloroform, any opinion of mine as to its merits is not entitled to much weight. It seemed certainly in several of my cases to prolong the interval between the pains, and thereby retard the termination of the labour; and I thought also that the uterine action was sometimes less effective when the patient was under the power of the medicine.—Yours, &c.

GEORGE STRELE.

From Dr Paton, Dundee.

I have ascertained that chloroform has been employed in upwards of fifty cases up to this date (5th June 1848), several of them instrumental, and with the best effect; and no bad consequence has attended it either to the mothers or children. All the children have been born alive. In the cases in my own practice in which I have used it, it speedily induced the anæsthetic state, and appeared to accelerate the uterine action in some. The recoveries of the mothers were more favourable than in former instances where no such agent was employed. In all, the children were born alive, except one, where the child was *acephalous*.

To explain the small number of cases in which it has been used here, I ought to mention that, in consequence of the deaths of two ladies in a respectable rank of life, from puerperal fever, when it was epidemic a few months ago—in both of which cases chloroform was used—the public, of course, attributed the unfortunate result to the new agent; and since then it has been difficult, and in many instances impossible, to overcome the prejudice against it. Of late, however, from the favourable opinion entertained of it by those who have had it administered, it is not so frequently objected to.—Yours, &c.

GEO. PATON.

Dr Anderson, president of the Medico-Chirurgical Society of Glasgow, and one of the most experienced and esteemed physicians in that city, some time ago wrote me a letter, from which I make the following extract:—

Not being connected with any obstetric institution, and having now little practice of this kind amongst the poor, I am sorry I cannot offer you any sufficient statistics on the use of chloroform; but I have had so much reason to be satisfied with its effect that I almost always use it, and most of my patients have expressed themselves as strongly in its favour as you have done. I recollect only two exceptions, where, although the usual effects were produced, both patients complained that, previous to the full anæsthetic effects, their feelings, instead of being agreeable, were very much the reverse. One

lady said, that she felt it like approaching insanity; and that she would rather endure the labour pains than be subjected to this again. In no case have I had reason to suppose that mother or child suffered injury. All the mothers have done well, and several of them have spontaneously remarked, that they made better and quicker recoveries than after former accouchements. In this I coincide. Two children were still-born. One, in the sixth month, died during parturition, after long-continued discharge of the liquor amnii for many weeks before the induction of labour; the other, in the eighth month, after hemorrhage from fatigue, followed by protracted labour: in this case a placental clot was found after delivery, so that the child's death was sufficiently accounted for.

On the whole, my experience of chloroform in parturition is more favourable than in other cases, where I have several times seen it produce alarming spasmodic and other nervous symptoms, followed by sickness. These effects, I think, are most apt to occur in subjects who have had spinal irritation, or an excitable state of the nervous system, or who use the chloroform when exhausted, or after abstinence from nourishing food.—Yours, &c.

A. D. ANDERSON.

The following communication was sent me in June last—

By Mr Spencer, Douglas, Isle of Man.

I forget how often I had used the chloroform when I last wrote you; but, since then, I have used it frequently, with invariable success so far as it is concerned. There have been no still-born children from its use, and all the children have continued to thrive well; none as yet have had a fit from teething—this I consider a great matter. We want some powerful proof here to bring it into general use. I cannot use it in half my cases, as I would not do it without leave from the patient. I had a case of puerperal mania the other day, and did *not* use it in it. If I had used it, I should have been blamed. This is so small a place that every thing one does is at once known. I have often operated under its use, with no ill effect.—Yours, &c.

J. H. F. SPENCER.

From Mr Ceely, Aylesbury.

I have much pleasure in communicating to you the general results of my very limited use of anæsthetic agents in midwifery. I have used both ether and chloroform in numerous operations, small and great, in public and private practice, have been several times the subject of their influence, and have repeatedly employed them in hysteria, epilepsy, and other nervous and spasmodic affections; but my employment of them in midwifery has been more limited, and at present restricted chiefly to primiparous and difficult or tedious cases. I have used them in only six cases as yet.

It has happened that most of my late obstetric cases have been so easy and so rapid as to supersede the use of chloroform. My own observations of its effects in midwifery, would induce me to *urge* its use in all *severe, protracted, or operative* midwifery; and in any case of natural labour, with only the ordinary suffering, I would not hesitate to employ it, with the concurrence of the patient or friends, if I saw no contra-indication; because I believe that, in proper doses, with suitable management, it is harmless and may be beneficial; but if the patient, under these circumstances, were averse, I should not, of course, press it. Neither do I think I should venture to employ it in natural and easy labour, where I suspected or knew that subsequent hemorrhage would arise, from flabbiness of fibre and flaccidity of the organ; because I think the *secale cornutum*, during the last hour or two of such labours, has in my hands been so beneficial, and seems more appropriate.

In all the cases in which I have employed ether or chloroform, the patients and their friends have been perfectly satisfied and gratified with the results, and abundantly thankful for the boon. In only one case has there been any

sickness or syncope. In a town about twelve miles hence, it has also been successfully used by Mr Knight of Brill, in several cases. In Bicester, I have heard of its use, but chiefly by younger medical men.

I will only add that, in using the chloroform, I have, in some of the cases, now and then omitted to repeat the dose, to show the friends and attendants how differently the patient was situated; but the latter has soon removed any hesitation of the friends by calling out hastily for that "nice chloroform" again.—Yours, &c.

ROBERT CREELY.

In a printed pamphlet¹ obligingly furnished me some time ago by Mr Stallard, surgeon to the Leicester General Dispensary, that gentleman observes:—

I have exhibited the chloroform in upwards of thirty cases of midwifery during the present year, and they have included a greater number of severe cases than the general experience would support; this is accounted for by the fact that two of the worst were pauper patients, to whom the surgeon's attendance is required only in cases of unusual severity. One other formidable case also occurred to my father, and in it I was requested by him to use the chloroform. In this experience I have never once observed the least retardation of the parturient paroxysms, *so long as absolute insensibility was not induced*; and in several cases the pains were palpably increased. When a patient is suffering acute pain, she does all in her power to suppress it, and the act of doing so is well known to retard the process of parturition; when, therefore, the pain is no longer felt, the effort to suppress it no longer ensues, and the accouchement is accelerated in a remarkable degree. But again, it has been argued that the pain of labour is desirable; and that it is wrong to interfere with this natural indication of what is going on. Now, with this assertion I am totally at variance; there is nothing so depressing to the powers of life; nothing so soon exhausts the already feeble body; nothing so effectually prevents the sufferer from aiding nature by her volitional effort, as pain; and consequently nothing so powerfully maintains the strength, or invigorates the system, as its annihilation. In the thirty cases I have attended I have not had a single case of flooding, and two individuals had never been free from it on former occasions. This is, in my opinion, an additional recommendation for its exhibition; and in all those excitable persons in whom flooding is most likely to occur, the diminution of fear and excitement caused by its exhibition, has a direct tendency to prevent the flooding. Nothing but a very extended experience can justify any conclusion upon this head; as far as my own experience goes, I am decidedly of opinion that chloroform exerts no perceptible influence upon the child.

From Dr Protheroe Smith, London.

I have records in my own practice and that of my friends of upwards of 125 cases of anæsthetic labour; and, with one exception, all have done well. In several thus treated, no hemorrhage has ensued, though in previous labours there was flooding. In nearly all, the getting up has been more speedy, requiring no aid of opiates and purgatives; and it is my sincere conviction that chloroform lessens the chance of puerperal inflammation and fever. I usually employ my inhaler to administer it, and with it I fancy I can administer it with greater success and precision. I have kept patients under its influence from half an hour to twenty-eight and a half hours. I have used it in cases in which bronchitis was present, and one lately, in which, at the time, there was complete aphonia. She has never had so good a time—in four or five previous births she had protracted recoveries, floodings, peritoneal attacks, &c.; these she has wholly escaped. I have used it in turning and forceps cases, &c., and in

¹ Practical Observations on the Administration and Effects of Chloroform in Natural Labour, p. 16, &c.

all I have been most successful. In administering chloroform I adopt your plan of inducing rapidly complete sleep, and afterwards keep up the effect by repeating the inhalations at each recurrence of uterine effort. The other day I had three cases, the one after the other, which strikingly illustrated the various effects of the chloroform on different subjects,—1st, a primipara, æt. twenty (administered in the second stage), kept under its influence four and a half hours, slept sweetly without any movement of the body, save during the last expulsive efforts, when the usual abdominal muscles were called into action. The “pains” under the chloroform became more frequent and powerful, and a fine male child was born. After the expulsion of the child, the patient awoke refreshed as from a sweet sleep, without any pain or evidence (save the advent of her first-born) of having been confined, as far as her own feelings were concerned. The uterus contracted firmly, and I instantly left her for the second—a lady of title. She required to inhale for a minute and a half (in the third stage), before the induction of anæsthesia was complete, when she became very talkative, addressing her maid in French, speaking of matters wholly foreign to her condition, and of an amusing character, as though she was perfectly at ease and had nothing to do with the labour, which advanced rapidly, and terminated in an hour from the first exhibition of chloroform. On recovery, in about three minutes after, she was introduced to a fine boy, and remarked she had been sound asleep. The third case had the chloroform in the *second* stage; she slept soundly in half a minute, and then became conscious of what was going on—described to the nurse who stood by, the progress of the labour, and observed that she was greatly delighted that she had her senses, and yet was wholly unconscious of any pain or distress of any kind. She remarked that with the entire freedom from suffering she was sensible of the progress of the labour, as though it was effected by an agency with which she was altogether unconnected. During the last three I made her sleep; so she was ignorant of the birth of a fine boy: in this and the second case the placenta immediately followed; the abdomen and uterus contracted firmly. In all three cases no after treatment was required, and they made rapid recoveries.—Yours, &c.

PROTHMER SMITH.

From Dr Rigby, London.

Of course I use the chloroform in midwifery, but I cannot think that a large dose at first, is the best way. In almost all private patients, you can draw the line between loss of pain and loss of consciousness. I give but a small quantity at a time; the patient takes it herself (on a strip of sponge), and when it is exhausted she asks for more.—Yours, &c.

E. RIGBY.

From Mr Lansdowne, Bristol.

I have now used ether or chloroform in seventy-one midwifery cases; I have two modes of administering it, the one with a bladder, in which is placed a brass pipe with a stopcock, and into this is screwed, after I have poured the chloroform into the bladder, a piece of elastic tubing with a mouth-piece, the whole being pierced with a bore $\frac{1}{2}$ of an inch, through which the vapour can be readily inhaled. If I find I am likely to be giving the chloroform for a long time, I use this apparatus, both for the sake of convenience, and also of economy, as 3j will last me nearly or quite an hour with this; and, should I use it many hours, it not only effects a great saving of material, but does not so frequently require replenishing, and is always ready at the approach of each separate uterine action; and it may (as has been the case with me) be used by any friends, or by the nurse, should the practitioner require to be absent for a short time. The other apparatus is an inhaler, such as is commonly sold; it is made of a thin and pliable lead, adapted over the nose and mouth, having a piece of perforated zinc in its front, and containing a piece of sponge, over which the chloroform is thrown; the depth of this inhaler is such as to prevent

the nose being touched by the chloroform. It is home manufactured, not expensive, and very easy of construction. This latter I make use of if I am likely to be wanting it for a short time only; it requires to be supplied afresh every five or ten minutes, and, accordingly, I use 3j or 3ss, which latter is my quantity when about to extract a tooth. If the action of the uterus causes great pain, as is frequently the case in an early period of the labour with the first child, I commence giving it as soon as the os uteri is sufficiently dilated for the head to pass; I have given it when the opening has not exceeded the size of half-a-crown. I believe it may be given with impunity as early in the labour as we please, and the only obstacles to its being so used that I can see, are the inconvenience to the medical attendant in being thus occupied with one patient for such a length of time, and also the very great expense which such a lengthened use of it must entail. On the patient's account, I can see no possible reason why it may not be used for a whole day, or even more; indeed, I cannot see why a limit should be set to the length of time in which it may be used. I have no doubt but that it will soon be the anodyne generally used at the latter stages of painful cancerous diseases. The greatest length of time in which I have used it has been 16½ hours, a fresh inhalation being made at every renewal of the action of the uterus; in other cases, I have given it 11½ and 12 hours, and the only reason of the inhalation being limited to this time, has been the cessation of the necessity for its use, namely, that the child has then been born, otherwise it would have been continued until such event had taken place.

I have found that nearly all my patients have recovered very rapidly; most of those who have had children previously, have been astonished at the unusual rapidity of their recovery.

I find no difference as to the expulsion of the placenta and the subsequent discharge, when administering chloroform, to what takes place in the usual natural labour. I have, upon two occasions, used it for very severe after pains, pains so severe that their cries could be heard at a considerable distance; indeed they appeared worse than the pains of actual labour; in both cases the pain was completely subdued by its use. Both these persons had determined not to avail themselves of the benefit of the chloroform during labour, neither did they, but they were delighted afterwards with its soothing effects. The former of these I had long resolved to give it to for this express purpose, it was her thirteenth child; her labours have always been very rapid, scarcely any pain accompanying them; but no sooner has the child been born than her agony has been almost past bearing, the pain recurring at intervals for a fortnight. Upon this occasion I gave it to her three times within the first ten hours, and she had nothing to complain of afterwards.

As regards sickness, I have not found that symptom, except where fluid has been previously taken; on the contrary, if the patient has been sick the chloroform has almost invariably checked it. The cramp I have not heard them suffer from whilst under its influence. I have never yet met with any thing which has caused me to regret having used it.—Yours, &c.

J. G. LANSDOWNE.

During the course of last winter and spring the practice of anæsthesia in midwifery was tried in London by a very intelligent young Russian physician, Dr Haartman of Hessingfors, while, for the sake of information, he was living as house-surgeon in the great Lying-in Hospital at Westminster. Dr Haartman subsequently visited Edinburgh, and kindly drew up for me, before leaving England, the following account of the results of his practice. I have much pleasure in publishing his letter, both on account of its own intrinsic value, and because the results of the practice of anæsthesia in this Hospital have been in no small degree misrepresented, both publicly and privately.

Results of Anæsthesia in the Westminster Lying-in Hospital.

I fulfil with the greatest pleasure your wish, in writing down my observations on chloroform as used in midwifery. I do it certainly not without much embarrassment, the greater the more I think of my ignorance of the English idiom; but, encouraged by your indulgence, I do not hesitate to begin the task, more especially as I know that some erroneous ideas of the use of the chloroform in the General Lying-in Hospital in London have been spread about.

During the three months I was house surgeon in that hospital, I had the opportunity of observing 105 women in labour, of whom about twenty-five were attended by my friend Dr Delafield of New York, the other eighty by myself. But having been most of the time the only house surgeon, and otherwise much engaged, I was not able to exhibit the chloroform oftener than in twenty-five cases, which, I need scarcely say, were in no way selected. The most of these patients were, however, *primiparæ*. In the administration of the chloroform I followed your method, using simply a towel, upon the interior of which the liquid was diffused; but I held it as a rule to exhibit it in the beginning of each case rather in small doses, and with caution, watching, in the mean time, the pulse as the surest indicator of any danger. But when the patient got accustomed to it I gave it freely, not removing the towel till the patient was in deep sleep. In this state, the respiration was in general sonorous, sometimes stertorous; the pupils were usually somewhat contracted, sometimes, however, dilated, sometimes not at all affected. The pulse was, in the beginning, either a little accelerated, or quite natural, I think, when the patient was not frightened. Yet, I have seen the pulse without any bad consequence go down to 40, when the chloroform was used for a long time or in large doses. Its strength was very seldom altered, being then weaker. The patient usually lay quiet, all the muscles being relaxed till the uterine contractions came on, during which they all, with few exceptions, pressed down as in common labour. Some of them complained, although they afterwards said, "they felt no pain whatever." Few talked or laughed, except the Irishwomen, whom I found rather refractory to the anæsthetic influence, probably because they are, in general, too much accustomed to drinking spirits. All the cases were, with few exceptions, observed with the greatest care, and both the duration, the frequency, and the nature of each uterine contraction, both before and after the chloroform was exhibited, were marked down. Thus, I find, by comparing all my annotations, arranged in tables, that, during the full effect of the chloroform, the uterine contractions became less frequent and, I should say, less powerful, but, when the effect had passed off a little, they then became more frequent and shortened in duration; for example, if the interval between two uterine contractions before the use of the chloroform was 6 minutes, and the duration of each $1\frac{1}{2}$ minute, both these periods were reduced after its exhibition. The interval, from 6 to 5, 4, or 2 minutes, and the duration of each pain from $1\frac{1}{2}$ to 1 or $\frac{1}{2}$ minute. Besides, I think, that in this state the uterine contractions in general came on more suddenly than in common labour, a circumstance which, however, may depend on an illusion, by the absence of pain existing and announcing the action of the uterus in the usual labour. I have never observed the uterine contractions quite arrested, yet often somewhat delayed. The supposed relaxation of the soft parts seems to me at least uncertain and accidental. In two cases in which I thought the chloroform had very much relaxed the rather rigid parts, I had, notwithstanding, rupture of the perineum. Among the twenty-five cases of chloroform, I had only in one a slight hemorrhage, a fact the more remarkable, as at the epoch at which I used chloroform most frequently, I can say I had hemorrhage, and that often very severe, in almost all the other cases in which chloroform was *not* used.

After the delivery, I found the patient in general quite unconscious of any

occurrence during the anæsthetic state, some of them saying, nevertheless, that they had felt something going on, although they did not know what. They seldom complained of headache, and it usually disappeared during the following day. The mothers recovered all speedily and perfectly, with one exception worthy your attention, although I am fully convinced that the accident by no means was the result of the chloroform, of which only two drachms were used—my provision at that time not being larger. During the use of the chloroform this patient was, as usual, insensible and quiet; but, for want of chloroform, the effect could not be kept up for a long time, and she was delivered with the usual pains. She was the following day perfectly well, and continued so till the 8th day after the delivery—she had then complained to the other patients of headache. I was, however, not called for before she, in the afternoon, had had a fit of what is commonly called apoplexia nervosa; after which she got paralytic on the right side. I need not to give you a tedious description of the proceeding, and the treatment of the disease; allow me only to state that the patient began, by the use of strengthening medicine, to walk about in a month.¹

Of all the 105 children, six were either still-born, or died sooner or later after the birth—two only of them belonging to the twenty-five cases of chloroform. But, before I relate these cases, I beg permission to point out some general remarks. In the majority of the cases I could not discover any change whatever upon the child, some of the children being rather bloodfull, others anemic—states depending, I think, as usual, on the duration and the nature of the labour. In one case, however, in which Dr Ferguson was present, the newly-born infant was found rather strange, or, to use the eminent doctor's expression, "tipsy." It breathed less frequently and more abruptly than usual, the sound of its scream being rather singular. It recovered, nevertheless, perfectly. I must here observe that the mother (Irish), who, after the confinement, had brought up a large quantity of gin and broom, confessed having taken spirits before admittance into the hospital. Of the two children who died, the first was rather weak and thin, but continued pretty well till the seventh day, when it got peritonitis, and died on the tenth day after birth. The second, a large and fat child, died four hours after birth: in this case, when the head was born, I observed around the neck small blue spots, which I found to be blood extravasated under the skin. After birth the same kind of spots began to appear over nearly the whole of the body. The child breathed well in the beginning, but would not suck. It died without convulsions. By the *post mortem* examination, the spots were found to be blood, extravasated from the capillary vessels of the skin; a large quantity of half-coagulated blood was found in the cavity of the peritoneum, and small blood coagula in both lateral ventricles of the brain. The lungs and the brain were slightly congested. The blood seemed to be quite natural, and had no anormal smell.²

¹ The "puerperal paralysis" was in this, as it is in most other cases, probably connected with albuminuria. One of my patients, who was confined for the second time, four months since, was attacked two years ago with hemiplegia immediately after the birth of her first child. I have a patient at present under my care, from Forbes, who several years ago was attacked with hemiplegia at the time of delivery.

² This special case of purpura has been often mentioned. Every one acquainted with infantile pathology knows that purpura is not a rare occurrence at birth, as the cases and observations of Andral, Billard, Otto, Lobstein, and Graetzer, &c., amply prove. A patient who came some time ago from London to be confined in Edinburgh under my care, was gravely assured by a distinguished London physician, that if chloroformed, her child would be sure to be, as they all were in the Westminster Hospital, "either dead born, or with their blood in a putrid and dissolved state!" The want of the head in the anencephalic child born at Dundee (See Dr Paton's letter, p. 235), might have been as logically ascribed to the mother's use of chloroform, as the purpura in the above instance.

Before I finish, I beg to add that I have used the chloroform in a case of turning with the greatest advantage. I was called out from the hospital to a poor woman in labour, and found a presentation of the cord and the uterus so contracted that I could nowhere introduce my hand, although I made repeated attempts without success. I then gave a large quantity of chloroform, and I was quite astonished at the great change which took place, it being now uncommonly easy to perform the turning.—Yours, &c.

CHARLES DE HAARTMAN.

Of the use of chloroform in midwifery in Ireland, I have few details. From Dublin, my friend Dr Tyler wrote me in June last, that the practice has not yet been much tried. He observes—

As to the progress of chloroform in obstetric practice here, I regret to state that its virtues have not yet been fairly put to the test by any of our Dublin accoucheurs, owing to a dread of bad consequences resulting, although I am unaware of any case followed by such being brought home to it, except a rumoured one of *threatened* convulsions and spasms, where ——— was administering it.

As to the surgeons, I hear them all speak in the highest terms of it. The resident surgeon of Steven's Hospital, Mr Wilmot, told me yesterday that he administers it previously to every operation there, now in some hundred cases, without meeting with any untoward result.—Yours, &c.

A. TYLER.

Subsequently Dr Tyler informed me that Mr Shekleton, master of the great Dublin Lying-in Hospital, was trying the effects of chloroform. While this sheet was passing through the press, Dr Denham, assistant physician in that hospital, visited Edinburgh, and informed me that Mr Shekleton has now exhibited chloroform in upwards of forty cases of labour, most of them of an operative or instrumental kind, and that the effects have been such as to induce him to go on with the trial of it. No unfavourable results, either as regards the mothers or children, have been observed to follow the use of the chloroform. Some of the principal private practitioners in Dublin, are also, Dr Denham informs me, beginning to employ the practice.

Regarding the use of chloroform in midwifery on the Continent of Europe, I possess little or no information. Dr Krieger of Berlin, tells me that in that city most medical men are opposed to the practice of anæsthetic midwifery, but still, five or six accoucheurs constantly make use of it. Writing to me in July last, Dr Krieger says—

About five or six accoucheurs of this place, I don't think there will be more of them, use chloroform in almost every case they attend; many more only in cases of morbid labour; the majority not at all. The cause of this curious occurrence—*curious*, because in *every* surgical operation chloroform is made use of—may be found, not only in the disinclination of the public at large to such extraordinary means in quite a natural process (as they take labour for), but perhaps also in the expense, chloroform being still a costly article. I don't know whether the preparation we get here is less strong than it ought to be, or not, but I seldom require less than six drachms or an ounce, sometimes more, for one delivery, and the price is as much as half-a-crown per

ounce at apothecaries' shops. Professor Martin of Jena has used ether in seven, chloroform in ten cases of morbid labour, and cannot strongly enough recommend the latter anæsthetic agent, stating, that he never witnessed but favourable consequences. I am sorry I cannot give you any more details about the progress chloroform has made in Germany—but the perplexing political affairs have so very bad an influence on scientific publications, that we get but very little medical news from any where on the Continent, and those only such as were long prepared before we fell victims to revolutions.—
Yours, &c. E. KRIEGER.

In a letter which I have lately received from Vienna, the writer, Dr Arneth, informs me that the use of anæsthetic agents in midwifery is, as a general rule, discountenanced in that city by the two leading professors of midwifery in the University, but that, in several late cases, they have used it in operative delivery. He further states, that in Würzburg the use of chloroform in obstetric practice is more common. He states—

While I am writing this letter, Professor Kiwisch of Wurzburg tells me, that in that place no woman is confined, neither in private nor in hospital practice, without having been chloroformed. In two cases of eclampsy he saw decided effect of these vapours, viz., the fits subsided.—Yours, &c.

F. H. ARNETH.

The preceding kind of evidence, in relation to the practice of anæsthesia in midwifery in this and other countries, might, as I have already stated, be very easily increased and multiplied by a little more extended inquiry, and by an appeal to the experience of the numerous accoucheurs here and elsewhere that have employed it. But the evidence as it stands—and spontaneous as it is in most cases—is amply sufficient to show both the great extent to which the new practice has already been adopted, and the great success that has attended upon it. Every innovation in medicine which implies, like the present, a violent and extensive change in existing doctrines and old established practices, has always been, for a length of time after its introduction, stoutly decried and resisted. The history of the first introduction and subsequent progress of the three greatest modern improvements in practical surgery, midwifery, and medicine—viz. the ligature of arteries, the induction of premature labour, and the discovery of vaccination—afford sad but strong historical proof of this observation; and we have many minor instances of the same constant enmity to change, in the bitter opposition to which the first employment of antimony, ipecachuana, cinchona bark,¹ and other

¹ The London physicians have, on several occasions, specially distinguished themselves by their determined and prejudiced opposition to all innovations in practice not originating among themselves. In the whole Pharmacopœia, there is perhaps no one remedy which, at the present day, is acknowledged to be of greater value, or to have saved more human lives, than cinchona and its preparations. In the seventeenth century, the proper time and manner of using the cinchona bark, for the cure of the then prevalent intermittent fevers of England, was made out by Robert Talbor, a medical practitioner in Essex. When Talbor subsequently removed to London, and began to use with success the new remedy

medicines encountered. And I believe that I am correct in stating, that probably no innovation, embodying so very direct and decided a deviation from all the former routine and rules of practice, as the employment of anæsthesia in midwifery implies, ever, in the same short period, made such extensive way and progress as it has done among the profession. As a matter of course, however, it has called forth also abundance of published and unpublished opposition and objection. No small share of the resistance against it has taken the form of personal or professional abuse of me as the introducer of the practice. All that I most willingly pass over and excuse, as, judging from all past experience in medicine, it was nothing more nor less than I was entitled to expect under the circumstances. But some difficulties and objections of a more palpable nature have been urged against the practice; and, in conclusion, I will very briefly allude to, and attempt to answer, the more prominent among these that happen to have been brought forward.

ALLEGED DIFFICULTIES IN THE SUPERINDUCTION OF ANÆSTHESIA.

In a previous page, I have stated the principal circumstances which require to be attended to in the exhibition of chloroform in labour. I have been occasionally told of cases, in which it was supposed that it was impossible to produce the anæsthetic effect of this agent. In my own practice (and I have now used chloroform in many hundred persons), I never yet met with a single instance in which a person was proof against its full influence.

It has been sometimes averred in the English journals, that on attempting to use chloroform, jactitation, incoherent talking and delirium, spasms, &c. &c., have supervened *instead* of a state of quiet anæsthesia. These are symptoms which do occasionally come on in the *first* or exciting stage of its action, more especially if strict

in the cure of the common agues of the metropolis, he found that, as he gained the favour of the world, he lost that of the physicians of London; and apparently their persecution of him became such that the King at last interfered, and in the year 1678, King Charles II. sent a royal mandate to the College of Physicians, commanding them not to give Talbor "molestation or disturbance in his practice." Among the list of London physicians averse to the new practice of curing ague by cinchona bark, De Bergen mentions the illustrious names of Sydenham, Harvey, &c. In 1698, a Dutch physician, Dr Groenvelt, published a work entitled, "*De tuto cantharidis in medicina usu interno.*" A few years previously, viz. in 1693, when Groenvelt practised in London, the President of the College of Physicians imprisoned him in Newgate for daring to recommend and use the new remedy whose virtues he had discovered. Six or seven years after vaccination began to be generally used throughout England, Dr Moseley, a member of the London College of Physicians, suggested to his College the propriety of putting down "the beastly new disease," as it was termed, of cow-pox; and in 1805, he boasted that the middle and inferior classes of London had then "renounced the delusion." In the last Number of a respectable London medical journal, a London medical practitioner questions whether the practice of relieving women, by anæsthetics, from the pains and agonies of parturition, should not "be considered criminal according to law!" See *London Medical Gazette* for September 8, p. 424.

quietude is not enjoined; and, though they are apt to terrify the beginner, they are in reality no more serious in their effects and character, than some of the equally frightful symptoms sometimes seen in hysteria. They are an evidence, however, of one of two things; either that the vapour is being given too slowly, or that it is given in too small quantity,—in an exciting, in fact, instead of a soporific dose; and the simple remedy, as every one properly experienced in its action knows, is at once to increase the dose in order to pass the patient as speedily as possible into the *second* or full narcotic stage.

Chloroform, it is alleged, sometimes gives rise to much coughing, and pulmonary irritation. Certainly not so, if the chloroform is of good quality, and its vapour is not at first approached in too strong and concentrated a form to the face of the patient. After some experience, it will be found that it can be given so as seldom or never to induce even coughing. Some time ago, a well known physician, in a large city of the south of England, wrote me, that he and his townsmen had found it too dangerously irritant a substance to breathe, and that he had seen it produce cough, bronchitis, phthisis, &c. The answer was simple; it never produced any such effects in Edinburgh practice. And I believe that the explanation was equally simple; he and his townsmen had experimented with an imperfect and impure article. A few days ago, one of the principal druggists in Edinburgh showed Dr Christison, Dr Douglas Mac-lagan, and myself, a bottle of chloroform of high specific gravity, viz. 1490, which he had just received from a very large manufacturing chemical house in London. It was impossible to breathe it without feeling great irritation in the throat and chest. It emitted fumes that at once reddened litmus paper; and which, on examination, proved to be muriatic acid. Is it wonderful that bronchitis, coughs, and more serious disasters, should have followed the inhalation of such an improper and dangerous article?

Dr Letheby has shown, that some kinds of chloroform in the market, besides containing muriatic acid, are also mixed with aldehyde, hydrochloric ether, hypochlorous acid, &c.¹

¹ See *Medical Gazette* for June 16, 1848, p. 1038. The presence of some of these deleterious agents has been supposed to be an inevitable and speedy effect of the spontaneous decomposition of very pure chloroform. But I find that some of the article, manufactured several months ago in its purest form by Duncan, Flockhart, and Co., of this city, has undergone no kind of change, even though long exposed to the sun. Messrs Smith have also shown me the same, in regard to their chloroform. I have reason to know, that the dangerous article alluded to in the text as containing a quantity of muriatic acid, has been extensively offered to the profession, at a price two or three shillings per pound cheaper than is charged for the pure chloroform manufactured by other houses; and, probably, its very cheapness has led to its extensive use. The following is the formula by which chloroform is prepared by Messrs Duncan, Flockhart, and Co. of Edinburgh, whose article I have always found of the most superior quality:—4 pounds of chloride of lime, and 12 pounds of water, are first well mixed together, and then 12 ounces of spirit added. Heat is then applied to the still (which ought not to be more

OBJECTIONS TO ANÆSTHESIA IN MIDWIFERY.

Objections of various kinds, religious, moral, and medical, have been zealously brought against the practice of anæsthesia in midwifery.

Elsewhere¹ I have attempted to answer the supposed religious objections that were at first so very strongly urged in various quarters against the practice, on the supposed ground of the permanence of the primeval curse; and I have shown that the disputed word "sorrow," *'etzeb* ("in sorrow thou shalt bring forth children"), does not in the original Hebrew really signify the sensations of pain, but the muscular efforts and contractions connected with childbirth. Besides, if this were not the fact, and that it was the duty of man to give effect to the curse, instead of struggling to ameliorate and resist its penalties and influences, then the whole art of physic should require to be abandoned entirely, for, in the primeval curse, man was doomed to die; and yet is not the great leading aim and object of the physician a continuous attempt to preserve him in life? All forms of obstetric assistance would require also to be rejected, for the whole art and science of midwifery is one undivided effort to abate and ameliorate the effects of the curse; and to attain that object imperfectly, as heretofore, by venesection, baths, by counter pressure to the back, and other minor practices, is as sinful as to attain it more perfectly now by anæsthetics, inasmuch as the principle of interference is not altered by the degree of relief given; "for whosoever shall keep the whole law, and yet offend in one point, he is guilty of all." In short, if there is any evidence of feelings of impiety and irreligion in the whole question, it is surely on the side of those persons who suppose that pain is permanently ordained in the primal curse as an accompaniment of human parturition; and yet that by anæsthetics, man, the creature, has discovered a power by which he can alter and subvert an immutable decree of God, the Creator.

The principal moral "objection," as it has been termed, against the employment of anæsthesia in midwifery, amounts to the often repeated allegation, that it is "unnatural." "Parturition," it is avowed,

than a third full), but as soon as the upper part of the still becomes warm, the heat is withdrawn, and the action allowed to go on of itself. In a short time the distillation commences, and whenever it begins to go on slowly the heat is again applied. The fluid which passes over separates into two layers, the lower of which is chloroform. This, after having been separated from the weak spirit forming the upper layer, is mixed with half its measure of strong sulphuric acid, added gradually. The mixture, when cool, is poured into a leaden retort, and distilled from as much carbonate of baryta by weight, as there is of sulphuric acid by measure. The product should be allowed to stand over quicklime for a day or two, and repeatedly shaken, and then redistilled from the lime. The specific gravity of the resulting chloroform is generally 14.96 or 14.97.

¹ See also Dr Protheroe Smith's late pamphlet entitled, "Scriptural Authority for the Mitigation of the Pains of Labour."

is a "natural function," the pain attendant upon it is a "physiological pain" (Dr Meigs),¹ and it is argued that it is impossible "to intermeddle with a natural function;" and to use anæsthetics is a piece of "unnecessary interference with the providentially arranged process of healthy labour" (Dr Ashwell).² The above is, perhaps, the most general and approved of all the objections entertained and urged at this moment against the practice of anæsthesia in midwifery. But it certainly is a very untenable objection; for, if it were urged against any of our similar interferences with the other physiological functions of the body (every one of which is as "providentially arranged" as the function of parturition), then the present state of society would require to be altogether changed and revolutionized. For the fact is, that almost all the habits and practices of civilized life are as "unnatural," and as direct interferences with our various "providentially arranged" functions, as the exhibition of anæsthetics during labour. Progression upon our own two lower extremities is a "providentially arranged" function, a "natural process;" and yet we "unnaturally" supplement and assist it by constantly riding on horseback and in carriages, &c. The "physiological process" of walking is apt to produce pain and injury of the uncovered foot of man, and we "unnaturally" use boots and shoes to bind the foot, and add to the protecting power of the cutaneous and other structures of the sole. Mastication and digestion are "natural processes;" but we daily intermeddle with and attempt to aid them by the arts of cookery and dietetics; and so on with regard to other functions.

To annul the pain of labour by anæsthetics is, argues Dr Meigs, "a questionable attempt to abrogate one of the general conditions of man." Riding and railway travelling abrogate one of the general conditions of man (progression), and are constantly leading to accidents and deaths. Should we never travel therefore except on foot? Disease and death itself form one of the most "general conditions of man,"—and medicine is a "questionable attempt to abrogate them." Should medicine therefore be at once abandoned?

In a note now lying before me, an eminent London divine urges the following objections against anæsthesia either in midwifery or surgery; and I notice it here, because it is an objection which I have often heard repeated. He writes:—"The question with me is not the alleviation of pain, but the destruction of consciousness. I should hesitate greatly to take a step which destroys consciousness." Now, certainly, our consciousness is destroyed in natural sleep as much as in the anæsthetic sleep. I have little doubt that the distinguished writer whom I have quoted, has many a day, perhaps during almost every day for a long lifetime, voluntarily given up and destroyed his own consciousness in sleep, for an hour or

¹ Philadelphia Medical Examiner, March 1848, p. 152.

² Lancet for March 11, 1848, p. 291.

two longer each morning than the necessities of his system required. Putting these many hours together, he has perhaps now, from first to last, unnecessarily but voluntarily, surrendered up his mental consciousness for periods that, if added together, would count up weeks, and months, and perhaps years. He has done so too, merely for the reprehensible indulgence of indolence; and yet he insists upon his fellow creatures not surrendering up *their* consciousness for a short time on rare and extraordinary occasions, when the object is the far more legitimate one of the avoidance of unnecessary physical pains, and the securing life and health by saving the system from the endurance of these pains. If we may sleep, and thus indulge in the destruction of consciousness to avoid and cure corporeal fatigue, surely we may do the same to avoid and cure corporeal agony.

Dr Merriman¹ opposes the employment of anæsthesia in natural labour, on the ground of "the great superiority of allowing nature to conduct the whole process of the birth." But the practice of anæsthesia does, in reality, allow "nature to conduct the *whole* process of the birth:" it merely abstracts that intensity of pain and suffering which accompanies the act of labour in the civilized woman, —a "disadvantage inseparable" from civilisation, to employ Dr Merriman's own expression, and which is not an essential part of the process of parturition, according to his own doctrine; for, as he himself states, "in the earliest ages of the world, and in savage nations at present, childbirth appears to have been, in almost every instance, *easily* accomplished; the mother suffers *little*." And in this state of natural anæsthesia the convalescence is consequently unusually rapid; for again, to quote Dr Merriman's words, she almost "at once resumes her ordinary occupations." Dr Merriman afterwards, in speaking of the use of chloroform, decries its propriety in any except "instrumental or very tedious labours,"—arguing that we should not interfere unless where the labour is morbid, for (to use his own words), "the duty of the physician is to *imitate* nature as far as possible, and watch her methods of acting." But surely the physician strictly imitates nature in her most natural state, according to Dr Merriman's own premises, when, during labour, he induces by art that state of anæsthesia, which, in Dr Merriman's opinion, originally pertains to parturition in the human mother. For the female in an uncivilized state more truly shows us the true method and types of nature, than the female in a civilized state. Besides, are we not called upon to relieve the woman, when we can, of her sufferings, as an act of common professional duty and common professional humanity? In law and in morals, we judge greatly of actions by their intent. No accoucheur would intentionally inflict upon a patient the agonies of labour by a deliberate act of *commission* on his

¹ Arguments against the Indiscriminate Employment of Anæsthetic Agents in Midwifery. London, 1848.

part. Is an accoucheur properly justified in intentionally refusing to save a patient from the agonies of labour by a deliberate act of *omission* on his part? When a child, at birth, is intentionally destroyed by the loss of blood, it does not matter, in the eye of the law, whether the death has been produced by voluntarily omitting to tie the umbilical vessels, or voluntarily opening other vessels.

Up to within the last few months, and till the power of annulling pain by the inhalation of ether was discovered, severe bodily pain, such as we witness in surgery and midwifery, was universally regarded by the profession as possessing an evil and morbid effect. Some of the opponents of anæsthesia have taken up a novel and different view; and, as a medical argument against the practice of anæsthesia in midwifery, it has been particularly averred that a labour pain is "a desirable, salutary, and conservative manifestation of life force" (Dr Meigs). Parturient "pain is (says Dr Copland)¹ often salutary as respects its effects; neither its violence nor its continuance is productive of injury to the constitution," &c. No opinion, I believe, could be more erroneous. I have already shown in this Journal, from the evidence of extensive statistical returns, that some of the graver operations of surgery are now much less fatal in their results when patients are operated on under the condition of anæsthesia, and consequently without any attendant pain, than the same operations were formerly, when patients were submitted to all the agonies of the surgeon's knife in their usual waking and sensitive state. The prevention of the pain in surgical operations is, in other words, one means of preventing danger and death to those operated on; the saving of human suffering implies the saving of human life. And what holds good in relation to pain in surgery, holds good in relation to midwifery. Pain, whenever it is great in degree or great in duration, is in itself deleterious; and by shielding our patients, by anæsthetic measures, against the more severe portion of the pains of parturition, we not only preserve them from the agony of their more immediate sufferings, but we preserve their constitutions also from the effects and consequences of these sufferings. And the evidence which I have adduced in the preceding pages, that, when thus freed from the endurance of pain by artificial anæsthesia, they assuredly, as a general rule, make both more rapid and more perfect recoveries than when such means are not used; just as woman in a savage state, and where she enjoys a kind of natural anæsthesia during labour, recovers more easily and rapidly from the shock of labour than the civilized female. In short, in cancelling the pains of parturition by anæsthesia, we also, I believe, to a great extent, cancel the perils of the process; for the mortality accompanying labour is regulated principally by the previous length and degree of the patient's sufferings and struggles. In the Dublin Lying-in Hospital, when under Dr

¹ Dictionary of Practical Medicine, Vol. III. p. 484.

Collins' able care, out of all the women, 7050 in number, who were delivered within a period of two hours from the commencement of labour, 22 died; *or 1 in every 320*. In 452 of his cases, the labour was prolonged above 20 hours; and of these 452, 42 died; *or 1 in every 11*,—a difference enormous in its amount, and one surely calculated to force us all to think seriously and dispassionately of the effects of severe suffering upon the maternal constitution.

The last and certainly the principal objection against anæsthesia in midwifery, is the supposed danger accompanying the exhibition of anæsthetic agents. In the earliest paper which I published on the subject of chloroform in this Journal, I pointed out this circumstance in mentioning various cautions in the use of it. When we consider the immense extent to which it has already been employed in all quarters of the world, in medicine, surgery, and midwifery,—the little care sometimes observed in its use,—and the deleterious and dangerous articles with which, as we have seen (p. 245), it is sometimes mixed, the wonder is that so few alleged accidents have happened from its employment. By saving a vast amount of human suffering, it has already, I believe, been the means of saving no small amount of human life; and it is assuredly improper to argue, as some have done, that the mere chance of its disagreeing with some rare and special constitution, now and then, is any valid reason for refusing its use for the abatement and abrogation of human suffering. If there were any soundness in the reasoning, a thousand things beside would require to be abandoned. Railways, steamboats, stage-coaches, &c., when used as substitutes for the natural and physiological function of human progression, are ever and anon attended with accidents to limb and life. But surely no one would, from this, maintain that these means of conveyance should, in consequence be abandoned. Many persons are annually drowned in bathing.—Should bathing, therefore, be prohibited, and this powerful means of maintaining and restoring health be entirely forsaken? According to the Registrar's official returns, a great number of lives¹ are lost yearly in England by the improper medical use of opium.—Should the use of opium, therefore, be given up? Patients sometimes sink under the depressing action of antimony, calomel, &c.—Should these valuable drugs, therefore, be banished from the Pharmacopœia? Many a patient has perished in consequence of venesection.—Should this operation be expunged from the art of surgery? From mistakes and errors, &c., in diagnosis and practice, medicine and surgery are sometimes the unhappy means of destroying instead of saving life.—Should these arts, consequently, be interdicted? Published works on medical subjects have sometimes led both pa-

¹ In 1840, out of every 1,000,000 living in England and Wales, 24 were poisoned by opium, and 22 by other medicines improperly given to children below the age of five years alone.—(See *Seventh Annual Report*, p. 82.) See Taylor on Poisons, p. 187, &c., for the great numbers destroyed in England by opium, &c., improperly given.

tients and practitioners into serious and fatal errors.—Should no medical works, therefore, be allowed to be published? Long ago Raynalde, in sending forth the first work on midwifery ever published in the English language, seems to have foreseen that, against the utility of publishing books on midwifery, the same argument would be used as we have found in our own day used against the application of anæsthesia to midwifery; and he has answered the argument in a style so earnest and apposite, that I shall quote this reply, by anticipation as it were, in his own words, in an abridged form:—

“Lo!—such is the light judgment of them, the which in every thing, whereof may ensue both good and evill, have alwaies their eyes waking and firmly affixed and directed upon the evill, picking and choosing out the worst of every matter, omitting and leaving to speake of the best, as the thing which were nothing to the purpose. If every thing in this world should be weighed and passed upon after this sort, then should we be faine to condemne and banishe those things farre from us, which are, at this time, accepted and taken for the most necessary, worthie, and of greatest price or estimation. To be short, there is nothing under heaven so good but that it may be perverted and turned to an evill use, by them that be evill and naught themselves, and so abuse it: ne is there anything so absolute and perfect, but by the occasion of the abuse thereof, at one time or other, may and doth ensue great danger and damage to mankinde? Fire and water be two right necessary elements to the use of man, without the whiche he could not live: yet by the meanes of them many a miserable deed hath been done and perpetrated. By fire hath bin consumed and devoured whole cities and countries; by water, swallowed and drowned infinite men, shippes, yea, and whole regions. Againe (he continues), meate and drinke, to the moderate users thereof doth minister and maintain life; and contrary, to the unmeasurable and unsaciate gourmands and gluttons it hath full many times brought surfeit, sicknesse, and at the last, death. * * * * But (he argues) should men, for the avoyding of the aforesaid inconveniences, and for the reasons above said, condemne and banish fire and water, or forsake their meate and drinke? No, it were but madnesse once to think it. Therefore I say, the judgment of that eye can never be equal and indifferent, which hath more respect and regard alwaies to the displeasures and hurts possible to happen (only through the misuse of a thing) then to the emoluments and profites daily and commonly like to ensue to the wel users of the same: and that of it selfe is good, is never to be disallowed for the sake of them that do abuse it. For to them that love good themselves, whatever it be, it is to them a sufficient matter and occasion therein to seeke the glory of God, and the only profit of their fellow Christian: And contrary, such as be of ill disposition, will in every thing, be it never so good and salutary, pick out matter of maintenance to their lewdnesse, turning matters of discretion to foolish and peevish prating contention.”—(*Prologue to Raynalde's Birthe of Mankinde*, Edition of 1628, p. 9.)

ARTICLE II.—*Observations on Gunshot Wounds, made in Paris during the Summer of 1848.* By EDWARD WATERS, M.D., F.R.C.P., Edinburgh, formerly Senior President of the Royal Medical Society of Edinburgh.

(Read before the Parisian Medical Society, August 9, 1848.)

A MORE extensive field for the observation of gunshot wounds than that presented by the Parisian hospitals, after the fatal struggles of

February, and more particularly of June last, has rarely offered itself to the profession, under equally favourable circumstances for study; and it is to be hoped that the opportunity thus afforded will be made available, in reducing to some general rules of action the discordant opinions which have hitherto prevailed in this department of surgery.

Different and even opposite methods of treatment have been pursued by surgeons of high reputation, and a discussion has been opened at the Academy of Medicine to enable them to develop their views, and record the results of their experience; a solution, however, of the numerous questions involved in the subject is not to be looked for.

The following communication is the result of observations collected in various services. In it an attempt will be made to estimate the value of certain modes of practice, and to throw light on some points, in connexion with gunshot wounds, respecting which diversity of opinion still exists.

CHARACTER OF THE ORIFICES OF GUNSHOT WOUNDS.

The profession generally, guided chiefly by the opinions of military surgeons, till within a recent period adopted the belief, that the orifice of entry of a musket or pistol ball was, in all but exceptional cases, distinguishable from that of exit; that the former presented a clean equal border, somewhat depressed, and more or less contused; the latter a more or less irregular one, projecting outwards, and evincing less contusion; further, the exit of the ball was supposed to be marked by a larger orifice than its entrance.

M. Nelaton agrees in opinion with the military surgeons; but insists on the different appearances which the orifices present, according to the period at which they are examined.

When the wound is recent the orifices offer notable differences; that of entry is depressed, ecchymosed, contused; that of exit is somewhat prominent and lacerated;—the former presents a loss of substance; the second a simple solution of continuity, the flaps of which, if brought together, would almost completely close the wound; the irregularity of the flaps and borders renders the latter orifice the larger, notwithstanding the absence of loss of substance.

After the lapse of some days the case is different; the eschar becomes detached from the orifice of entry, which is thus proportionately enlarged; on the contrary, the flaps, bordering that of exit, become partially united, and this orifice is diminished accordingly.

In a medico-legal light these distinctions possessed great weight; recent observations materially diminish their value. There can be no doubt that, where all the distinctive signs enumerated exist, their testimony is conclusive; but unfortunately it is found that, in a very considerable proportion of cases, some of the signs are absent, and under such circumstances there is abundant evidence to prove, that frequently no positive deduction can be drawn from those present.

Different from what occurs in general engagements, the combatants in the streets of Paris often saw the hand that struck them, and were thus enabled to state positively the point of entrance of the ball. On these data, tested by all the circumstances throwing light on the inquiry, such as a knowledge of the position of the patient at the time of being wounded, the direction of the wound, &c., it is clearly established that, in numerous instances, the orifice of entry exceeded in size that of exit, and that its form was more irregular. M. Roux stated before the Academy, that, in his cases, he observed about an equal number of variations on the one side as the other; an observation which may be extended to the practice of other surgeons. In a case of gunshot wound of the thigh with only one orifice, in the service of M. Boyer, the ball had traversed the muscular parts transversely at the upper third, and remained lodged beneath the skin on the side opposed to that where it penetrated. The ball, extracted by incision, was perfectly round, notwithstanding which, the orifice, that necessarily of entry, presented an irregular border. In other instances, in the same service, where the limb was completely traversed, not the slightest difference in character between the two openings could be detected on the most careful examination directed specially to that end.

M. Blandin appears to have been the first to question the correctness of the rule thus impugned; his experience in gunshot wounds has long convinced him that the aperture of entrance is, in all but exceptional cases, the larger, and experiments performed by him are said to have proved the accuracy of his observation. According to him, the ball where it strikes the skin being supported by parts more or less resistant, acts as a punch, and produces an aperture nearly equal to its diameter; at its point of exit, on the contrary, the skin gives way before it, stretches in virtue of its elasticity, and is only traversed when it has attained its utmost degree of extensibility; the loss of substance is thus rendered very trifling, and the aperture consequently smaller than that of entry. M. Gerdy maintains the same opinion; and in this case also it is founded not merely on the examination of the wounded, but on numerous experiments performed on the dead subject and on living animals; in some of these the two orifices were precisely similar. It may be objected that results obtained on the dead body and inferior animals cannot be considered an exact criterion of what occurs in man during life; it is difficult, however, to conceive experiments better devised to solve the question, and, when conjoined with the facts observed in the Parisian hospitals, they constitute a body of evidence, which seems to place it beyond doubt, that in a great number of instances the orifices do not afford conclusive characters by which that of entrance may be distinguished from that of exit.

No satisfactory explanation of these conflicting facts has been advanced; but it is not improbable that the distance at which the wound is inflicted may affect its appearance. Thus when the shot

is fired at a short distance, the wadding, being carried along with the ball, may render the orifice of entry larger and more irregular than that of exit, in consequence of the wadding having been deposited in its passage through the soft parts; when the shot is fired at a greater distance, the absence of the wadding may render the two orifices nearly or exactly alike; if the ball in its passage come in contact with a solid substance, such as bone, its form may be altered, and its exit give rise to a jagged opening with projecting borders.

When a ball is nearly spent, as shown by the distance to which it penetrates, where there is only one opening, this not unfrequently resembles a simple incision, a circumstance observed in some cases. That the point of exit should occasionally present a similar character is therefore easily explained.

ANALOGY BETWEEN GUNSHOT WOUNDS, BURNS, AND POISONED WOUNDS.

The older surgeons believed, that in gunshot wounds the balls, being heated by the deflagration of the powder, cauterized the tissues they traversed. The groundlessness of this belief was demonstrated by A. Paré in a very simple manner; he showed that a ball might be fired through a heap of powder without igniting it.

Rejecting the theory of combustion, M. Velpeau insists that burns and gunshot wounds occasion analogous lesions, inasmuch as the contusions resulting from the latter may be followed by all the effects of the former, from simple erythema up to death of all the structures implicated. One of his patients was struck by a musket-ball on the chest; the projectile caused no wound, but simply a contusion, terminating in the formation of an eschar: without information it would have been impossible to pronounce what had occasioned it. In a similar manner the comparison might be extended to the effects of cold, which is equally capable of causing identical phenomena; redness, vesication, ulceration, eschar, and death; the only difference being, that in this instance the effects are secondary, dependent on reaction, and not primary.

With respect to the opinion originated by Vigo, that these wounds are poisonous, M. Velpeau considers that, though as regards the theory it will not bear the slightest examination, still in practice it should not be altogether thrown aside, as after some days these wounds contain a sort of poison, in the putrilage resulting from the elimination of the eschar, and which is liable to be absorbed.

DILATATION OF GUNSHOT WOUNDS, UNCONNECTED WITH FRACTURE.

The practice of dilating the orifices and canals of all gunshot wounds once prevailed. John Hunter was among the first to oppose this useless and injurious proceeding, and the force of his judgment and reasoning speedily showed itself in the general adoption of

his views by British surgeons. Foreigners of eminence, amongst whom Larrey père, and Sanson, may be mentioned, followed in the same track; still, long after the abandonment of the practice amongst us, it continued to be inculcated on the continent, where the influential example of Dupuytren contributed powerfully to postpone the period of its rejection.

The confidence of the profession in its utility is now so completely overthrown, that any recapitulation of the objections to the proceeding is unnecessary. M. Baudens, the celebrated surgeon-in-chief of the military hospital of Val-de-Grace, declares the threatened tension, to obviate which the scarification was practised, to be exceedingly rare where the ball has traversed soft parts without producing fracture, and stigmatizes it as a scarecrow; his testimony is the more valuable as he once entertained a contrary opinion. Imbued with the doctrines imbibed during his studies in Paris, and acting under the dictates of what he considered to be an imperative duty, he, on his first arrival in Africa, dilated all the wounds, though not without feeling acutely for the sufferings he inflicted. While acting thus, a *voltigeur*, with the two thighs traversed by a ball at their superior third, without fracture, was brought to him. To effect dilatation large and deep incisions of the four wounds were requisite. His courage failed him; cold water dressings were applied; and, while dreading the consequences of his conduct, the cure progressed steadily and rapidly.¹ From that moment he abandoned the practice, and the propriety of his having done so will not at the present day be disputed. Several cases are recorded where flesh wounds treated on this principle have been observed to heal by the first intention; when this happens, it seems certain that the contusion, ordinarily produced by the passage of the projectile, cannot exist. It has been attempted to explain the occurrence by supposing, that in these examples the projectile had acquired an angular form, and consequently acted as a cutting, and not as a contusing body. A more simple explanation is based on the fact already recorded, that, when the force of the ball is nearly exhausted, its contusing power is lost, and it produces, notwithstanding its spherical form, a simple incision. This view derives corroboration from the circumstance, that the orifice of exit and the portion of the wound near it have been seen to heal by the first intention, while the remaining portion passed through the slower process of separation of a slough, suppuration, and granulation.

M. Velpeau denies that gunshot wounds can heal without the separation of an eschar. He asserts that all the tissues touched by the ball are disorganized, and must be thrown off, and that the premature closure of the external orifices, by imprisoning the putrid

¹ The conversion of M. Baudens, which shows to how late a period this practice was advocated in the surgical school of Paris, is recorded by Sir George Ballingall in his "Outlines of Military Surgery," 2d. ed. p. 216.

detritus, becomes a source of great danger. Of the applicability of this reasoning to the immense majority of cases, there can be no doubt; but the value of those of an opposite bearing is not on that account in any degree affected, and indisputable facts are mentioned by Larrey, Sir George Ballingall, Sanson, and others, where the cure was completed without any separation of eschar.

The utility of the rejection of scarification is not limited to the conditions already detailed. It has been extended with the best results to cases where important articulations have been traversed. The following occurred in the service of M. Jobert at St Louis:—

Salle St Augustin, No. 68.—Delevie, soldier, aged twenty-five, of good constitution and sober habits, wounded June 23, 1848. He was struck by a ball in the popliteal space between the heads of the gastrocnemius muscle; it traversed the articulation of the knee-joint and emerged in front, having passed through the patella at its inferior portion without fracturing it. The wound did not occasion any marked general disturbance; no examination by probe or otherwise was made in the tract of the ball; the knee was kept perfectly at rest, and dressed with cold linseed meal poultices, frequently changed during the day and night; horizontal position on a pillow; no splint. Slight effusion took place in the articulation; but the inflammatory symptoms were exceedingly moderate, and speedily got under. The patient suffered no pain; the orifices of the wound gave no evidence of suppuration, and on the 25th of July cicatrization was complete; there was no swelling of the joint, and he was allowed to move about with the assistance of crutches. The movements of the limb were preserved; extension of the leg was performed without difficulty, but flexion occasioned some uneasiness.

M. Jobert is of opinion that some slight adhesions exist within the joint, occasioned by the development of false membranes. In the wards of the same surgeon, two other cases exist in which the articulation of the knee-joint was traversed, where the same treatment has been equally successful. An analogous one is said to be seen in the service of M. Gosselin. A similar case happened in the practice of M. Vidal de Cassis, and is related in his work on "External Pathology."

A soldier, wounded in July 1830, had the articulation of the knee traversed by a ball, which entered in front above the patella and emerged posteriorly, immediately below the popliteal space. In consultation with M. Guersant *fil.*, he decided on not dilating the wound, and the patient was perfectly cured. There was neither local nor general disturbance; the patient affirmed that he suffered nothing from the wound, the progress to cure of which was remarkably simple. The suppuration was so trifling, that without the attention bestowed on the wound, it might have been considered to have healed by the first intention.

It seldom happens that balls traverse articulations without serious injury to the articular surfaces; and examples of such success are, therefore, necessarily rare. M. Vidal de Cassis was not acquainted with one at the time at which he wrote. In two instances of wound of the same joint in the service of M. Jobert, the result was not so fortunate; and in the wards of M. Velpeau there are also two examples where the suppuration is very abundant. The fortunate ter-

mination in the cases that have been mentioned is not the less to be attributed to the treatment; and the good effects of non-interference with cutting or other instruments, are thus strikingly exemplified, and afford every encouragement for the adoption of the same course under the like circumstances. Dilatation of a wound of a synovial membrane is at least as formidable as amputation.

Where, therefore, a ball has traversed an articulation without material injury to the bones,—injury of which would be betrayed not only by local, but also by sympathetic constitutional disturbance,—no exploratory attempts should be made, but the strictest repose enjoined, and appropriate treatment adopted, in order to ward off or diminish inflammation.

BALLS LODGED IN THE BONES.

Hôpital St Louis, service of M. Jobert. Salle St Augustin, No. 7.—Boy, aged sixteen, wounded June 23, was struck by a musket-ball near the inferior third of the right leg anteriorly; the ball buried itself in the body of the tibia, without otherwise fracturing the bone; no attempt at extraction was made; the ball remained in the bone; the external wound marched steadily towards cure; on the 25th of July cicatrization was complete, and a day or two subsequently the patient left the hospital. The integuments covering the tibia anteriorly in its superior half presented extensive cicatrices, the result of necrosis of a considerable portion of the bone, caused by the fall of a piece of iron on his leg. This was followed by a long suppuration, and the extraction of considerable fragments. The patient had recovered from the effects of this accident previous to the infliction of the present wound, which occupied a portion of bone exempt from and inferior to that previously implicated.

Some rare cases have been observed where balls have remained lodged in bones without causing any disturbance; a preparation exists in the Musée Dupuytren, in which a musket-ball is encysted in the substance of the tibia. The subject from whom it was taken was a French soldier wounded in 1814. Percy mentions the case of an old carabineer who carried a ball in the tibia up to the time of his death, a period of twenty-five years; a few others are on record, but their small number constitutes a *prima facie* argument in favour of the conclusion, that balls cannot occupy such a position without causing symptoms which demand their removal; numerous cases are related where, after a shorter or longer period, their extraction was thus necessitated.

M. Jobert maintains a contrary opinion, influenced by the observations of balls lodged with impunity in the bones of animals, the hippopotamus, elephant, stag, &c.; he maintains that the same may equally take place in the human frame, and consequently condemns their extraction, as causing unnecessary torment and retarding the cure, unless when they are superficially situated, and can be removed without difficulty. In this he is opposed to nearly all surgeons. M. Baudens unites with others in insisting on their extraction, and, where the forceps is unavailing, prescribes the application of the trephine. Dupuytren directs their extraction when practicable.

able, as it is only very rarely that they do not occasion grave accidents; caries and necrosis almost always follow. Guthrie establishes the same principle; according to him, their presence in the extremities of bones gives rise to caries—in the body of long bones and in flat bones, to necrosis.

The practice of M. Jobert in these circumstances, is based on so small a number of facts that it stands little chance of being adopted. Arguments, founded on what is observed in animals, are admitted to have but a very limited application to man; for, were it otherwise, ligature of the aorta would be proved to be a practicable operation. Blandin once failed in killing a sheep, though he opened the arterial trunks of all the extremities and the neck.—

TREATMENT OF FRACTURES OF THE EXTREMITIES IN CONNEXION WITH GUNSHOT WOUNDS.

Two distinctly opposite methods are pursued in the treatment of fractures caused by gunshot wounds, and these extremes are connected by the practice of those who follow a middle course. An examination of the results and merits of these different methods will now be entered on.

In order to facilitate the consideration of the subject, these fractures will be divided into, *first—simple*, the term being used not in the ordinary surgical sense, but as indicating that the bone has been fractured without the detachment of fragments; and, *secondly—comminuted*, where this complication exists.

The *second class* is that in which the difference of treatment is most striking, and will be first treated. It offers several points for examination. These are:—*a.*—Dilatation and extraction of splinters. *b.*—Dressing and apparatus. *c.*—Cases demanding amputation. *d.*—Cases demanding resection. These questions will be discussed separately.

a.—Dilatation and Extraction of Splinters.

The various circumstances under which the splinters of bone characterising the second class of cases present themselves, modify the treatment, and a division of them which indicates these circumstances is therefore indispensable; that of Dupuytren answers every purpose, and will be here followed. According to him, the fragments are of three kinds, namely,—primary, secondary, and tertiary.

Primary fragments are those which are entirely separated from the bone and soft parts during the infliction of the wound.

Secondary fragments are those which are not completely detached from the bones and soft parts, being still connected to the latter by portions of tendon, muscle, ligament, &c.

Tertiary fragments, those, namely, which result from the contusion of the bones by the projectiles in the portions which border the point of fracture, and which nature produces in virtue of a particular process, ordinarily exceedingly long in completion, and lasting some-

times ten, fifteen, or twenty years. Dupuytren relates the case of a soldier wounded in 1813, and admitted into the Hôtel Dieu in 1830, in consequence of tertiary fragments following a gunshot wound received at that period.

Among the surgeons of Paris, MM. Jobert and Baudens respectively occupy the extremes of the scale referred to as marking the treatment of these injuries, and are distinguished by the zeal with which they support their opinions.

In all gunshot wounds M. Jobert adopts a uniform and exclusive mode of practice; he deprecates all interference with the wound in every instance, even in cases of comminuted fracture. He not only condemns the extraction of splinters of whatever kind, and leaves their discharge entirely to nature, but interdicts formally all examination of the wound. He combats local inflammatory symptoms by antiphlogistic measures, and, when the tension becomes excessive, relieves it by two longitudinal incisions, from three to five inches long, and penetrating through the skin and cellular tissue, practised on each side of the orifices at a distance of about two inches;¹ he invariably avoids dilatation of the wound itself. He justifies this treatment by the advantage obtained in preventing the access of air to the fracture, with which that obtained from the extraction of the fragments cannot, he contends, be compared. He further maintains, that direct benefit is obtained from the presence of the fragments—from those even that are completely detached—in that they contribute to the consolidation of the fracture, and prevent the formation of false joints; and he believes that in many instances useful limbs may thus be saved which would otherwise be sacrificed. The patient, it is admitted, may occasionally suffer for a time from the presence of the necrosed portions; but these are gradually eliminated, and the limb ultimately regains its original power. In other instances, in accordance with his views in respect to the lodgement of balls in bones, he maintains that the necrosed portions may occupy the callus, and remain for an indefinite period without giving rise to any annoyance. To support the correctness of these views he adduces the following cases:—in one of these the patient was wounded in February. The humerus was fractured comminutively near its head; the ball passed out; no dilatation nor extraction of fragments was practised, but some portions of bone were removed during the suppuration. An enormous mass of callus is developed around the seat of fracture, and consolidation has taken place. The callus incloses portions of dead bone, and a fistulous opening on the outer surface of the arm communicates with the seat of fracture. The patient retains the use of his hand, but is unable to raise or support the

¹ When a wound heals with difficulty, in consequence of the ablation of a considerable portion of the skin, M. Jobert adopts the same practice. In a case recently in his wards, where the breast had been removed on account of a tumour, these incisions were practised with excellent effect.

weight of his arm, which he carries in a sling; he suffers but little, and his health is good. The remaining patient had the humerus fractured by a musket-ball at the upper third, during the days of June; no examination of the wound nor extraction of fragments was practised. Consolidation of the fracture is now complete, and the wound perfectly healed. The only deformity of the arm is occasioned by the exuberance of the provisional callus, which is believed to contain necrosed portions of bone. The patient continues to support his arm in a sling.

M. Jobert attaches great importance to these cases, regards the results as among the finest ever obtained in surgery, and as fully justifying him in not extracting fragments of any description.

M. Baudens pursues an entirely opposite treatment. In all cases of fracture caused by gunshot wounds, his first care is to ascertain the nature of the parts by direct examination; and if the existence of splinters be established, and there be any hope of saving a useful limb, he immediately places the patient under the influence of chloroform, practises the necessary dilatation, and extracts all the fragments of bone, whether primary or secondary; his object in this being to convert, as nearly as may be, these formidable wounds into simple ones.

The first authorities, among whom Dupuytren may be cited, have always ascribed the local accidents attendant on these fractures to the presence of the splinters, which, acting as foreign bodies, light up and aggravate inflammation. In several cases M. Baudens, guided by the character of the local symptoms, the tension and discoloration of the skin, inflammation in short putting on a phlegmonous appearance, assumed the presence of splinters, and confirmed his diagnosis by forthwith administering chloroform to the patient and extracting them; in such cases he then, with a razor, practised long superficial incisions, not embracing the whole thickness of the skin, in order to relieve the engorgement, and applied ice to diminish the action.

It is thus shown that M. Baudens extracts all foreign substances, and that he not only removes the primitive or detached fragments, but also the secondary, or those still connected with the soft parts. Till lately no difference of opinion existed as to the propriety of this practice in the first class; but with regard to the second the case was different. Dupuytren, who advocated the immediate removal of the first, inculcated in his lectures, that in the greater number of cases the second were not to be extracted, unless when the operation could be performed without danger of hemorrhage or severe pain, and then only when the fragments had been more or less detached by suppuration; that is to say, at the termination of a variable period, eight, ten, fifteen, twenty days, a month, or even later. M. Baudens removes them at once, and his long experience in Africa gives great weight to his opinions.

Two objections only are urged by Dupuytren against the latter mode of proceeding; one is the severity of the pain attendant on the

operation, and he has recorded a case where he was forced to desist from researches which he had commenced, owing to the sufferings of the patient. The anæsthetic properties of chloroform completely overrule this objection; it is only necessary to witness its employment in these cases, to acknowledge unhesitatingly its beneficial effects.

The remaining objection, that, namely, of the danger of hemorrhage, alone therefore demands consideration. This danger, as resulting from the extraction of the splinters, seems to be more apparent than real. In reference exclusively to fractures of the extremities, it is difficult to conceive any grounds for dread in this respect, where amputation would not be directly indicated; were it required, however, to proceed to their extraction in comminuted fractures of the thigh, the depth of the parts might seriously complicate and impede the operation. On the other hand, cases exist where the presence of the fragments seems to have been the cause of hemorrhage.

Case fifty-three, in the work of Dupuytren, supports this conclusion; it occurred in July 1830. A ball fractured the two bones of the leg at the superior third; an attempt to preserve the limb was determined on. The orifices of exit and entry were dilated, the limb fixed in a fracture apparatus, with the looped bandage, and laid on pillows. Every thing went on well; the fever was moderate; the patient suffered little, and a healthy suppuration established itself, which daily became more abundant. The general condition of the patient continued pretty good, no rigors, no diarrhœa, no colliquative sweating, no pain in any region of the body. Such was the condition the 16th day after the accident, when, after a dressing performed with the greatest gentleness and precaution, a violent hemorrhage of a bright red colour manifested itself, and amputation of the thigh was performed.

Examination of the amputated leg.—The tibia and fibula were fractured comminutively at their superior fourth; fifteen or twenty fragments belonging to one and the other of these bones, and almost all detached, occupied the seat of fracture; the soft parts were torn, destroyed, and reduced to a pulp; purulent collections existed between the different muscular layers; the anterior tibial artery was altered by putrefaction, and presented an opening at its passage through the interosseous ligament.

This is a most instructive case; the condition of the parts at the time of amputation, showed how delusive were the hopes of success which had been entertained during the treatment. The artery was either opened directly by the action of a splinter on its altered coats, or by ulceration resulting from the inflammation entertained by the fragments of bone.

Ligature of the femoral was agitated and rejected. The advocates of M. Jobert's treatment, may attribute the unfavourable result to the dilatation of the orifices, permitting the admission of air to the seat of fracture; in the following case this practice was not adopted, and yet the result was equally unfortunate.

One of the wounded of June was admitted into the hospital of St Louis, under the care of M. Malgaigne, with comminuted fracture of the humerus at its upper fourth, from a musket-ball. No dilatation nor extraction of splinters was attempted; there were two orifices. July 10th, considerable hemorrhage manifested itself, which was arrested temporarily by compression; it recurred again

and again, and the following morning disarticulation at the shoulder joint was performed. The patient was in a weak blanched state, the operation was executed while in bed, chloroform was administered; a very trifling amount of blood was lost, but death supervened during the operation; the union of the flaps was in consequence not effected.

Three other cases of secondary hemorrhage have occurred in the practice of this surgeon, where no examination of the wound had been performed, and all terminated fatally.

The necessity of examining the wound in these fractures was forcibly exemplified by its condition in the present instance; numerous detached fragments occupied the seat of fracture, the surrounding soft parts presented a disorganized, dark, pulpy appearance. A portion of the ball, much disfigured, was extracted from under the pectoralis major muscle.

M. Pelletan has recorded a case of hemorrhage on the 70th day, occasioned by the presence of a splinter.

These cases might easily be multiplied, but a sufficient number has been advanced to show that the occurrence of hemorrhage may with equal reason be urged against allowing the fragments to remain, as against proceeding to their extraction. Any dread from this cause, cannot be considered sufficient to deter the educated surgeon of the present day from a beneficial operation.

It may be argued that the secondary splinters, retaining connexion with the soft parts, and consequently not completely cut off from the circulation, are not incapable of preserving their vitality, and on that account should not be removed. Experience does not confirm such a conclusion; the inflammation which they excite speedily isolates them; the pus resulting from a chronic abscess may long rest with impunity in contact with the periosteum, but that accompanying active inflammation rapidly destroys it, the nutrition of the fragment ceases, and it becomes converted into a true foreign body. When comminuted fracture of the bone has occurred without breach of the tegumentary surface, where a cutting instrument has effected a separation of a portion of bone without destroying its connexion with the soft parts, where a compound fracture has resulted from an accident which has exerted no contusing power directly on the bone, we know that reunion of the fragments may take place. In comminuted fractures from gunshot wounds, the condition of the parts is in no way the same; the projectile exerts on the bones an action analogous to that observed on the soft parts; the contusion caused by the projectile adds to the disturbance occasioned by the fragments, and a high degree of inflammation is inevitably excited, which rapidly destroys their connexion with the living organism; the state of the parts in these fractures differs, therefore, from that existing in those produced by other means.

Experience fully convinced Dupuytren of the futility of expecting that the secondary fragments would retain their vitality; he asserts that their detachment is operated spontaneously, through suppuration

at a shorter or longer period, varying from eight days to even months. During this period they keep up a constant irritation, and aggravate the local symptoms; the general disturbance is known to be always in proportion to that of the local inflammatory accidents; moderate when these are calmed by judicious measures, but, on the contrary, of a formidable character when these are neglected, or badly treated, or treated ineffectually.

It is to obviate these inconveniences that Baudens and Nelaton advise the removal of these fragments, as well as the primary ones, in order to place the patient at once in a condition, which is otherwise only attained at the expiration of an indefinite period,—the period necessary for their spontaneous removal. The patient is thus in the best possible position for ultimate cure; the chief cause of local inflammation is removed; the general fever diminishes proportionally; the strength is not worn down by prolonged suffering and suppuration, nor the nervous irritability of the patient morbidly increased. Twenty-five cases in the wards of M. Baudens, in which he followed these rules, and which are all terminating favourably, attest the efficacy of the practice.

Another argument in favour of this practice may be drawn from the occurrence of tertiary fragments, as whatever tends to heighten the local inflammation favours their formation. M. Baudens proceeds to the extraction of these as soon as their existence is ascertained, and the operation practicable.

The question now presents itself, whether the success of M. Jobert, and those who side with him, is such as to overrule the treatment here approved. Statistics are not yet attainable, and it is probably by these that the question will ultimately be judged. The practice of M. Jobert already offers two successful cases, so called; in these, consolidation of the fracture has certainly taken place, but an enormous mass of provisional callus is formed, inclosing several necrosed portions of bone; these represent true sequestra, will necessarily play the part of foreign bodies, and probably, at a future period, become the cause of great suffering. In the mean time, the progress of the healing process within the provisional callus is unknown, and a considerable time must elapse before it can be ascertained. In the man wounded in February a fistulous opening exists, as already stated, in communication with the seat of fracture. M. Jobert believes that portions of necrosed bone, may, like balls, remain in the bones without injurious consequences; according to Dupuytren their sojourn in the midst of callus is one of the most common causes of deformity, and of interminable fistula. In certain cases no provisional callus forms, and it is only after the separation of the necrosed portions that consolidation takes place.

M. Malgaigne adopts the expectant mode of treatment at first; but when suppuration is established, makes frequent use of the probe and forceps, extracting splinters six and seven weeks after the infliction of the wound. In a case of comminuted fracture of the fore-

arm, no examination of the fracture was made on admission; fifteen days afterwards, splinters began to come away by the suppurative process; as late as eight weeks after the infliction of the wound, he dilated one of the orifices to facilitate the extraction of a splinter.

Other surgeons extracted immediately the most easily reached primary fragments, and renewed the examination of the wound, day after day, to extract those remaining, and the secondary as they became loosened; at each examination causing much suffering to the patient.

The foregoing facts and reflections are held to indicate, that in every case of comminuted fracture of the extremities, where an attempt is made to save the limb, extraction of all the splinters of bone is imperatively demanded.—(*To be continued.*)

ARTICLE III.—*On a Peculiarity of the Humerus and Humeral Artery.*

By JOHN STRUTHERS, Esq., F.R.C.S., Lecturer on Anatomy, Edinburgh.

A FEW instances have been recorded in which the humeral artery and median nerve continued downwards in a direction towards the internal condyle, above which, in order to gain their usual position in front of the elbow, they passed around a peculiar process, there developed from the humerus, as if for the purpose of affording them protection.

This singular arrangement has occurred comparatively so often of late in the subjects dissected in my rooms, as to induce me to call more particular attention to it, since it is one of some importance to the surgeon, and is interesting also from the analogy it presents to the usual arrangement of these parts in certain animals.¹

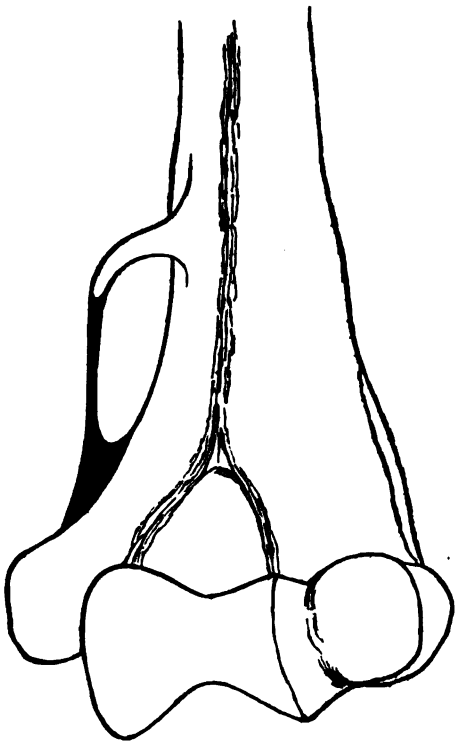
The process, and the ligament by which the protective arch is completed, are represented by Tiedemann (*"Tabulæ Arteriarum,"* Pl. xv. fig. 3. 1822); but, in this instance, the artery which passed through was not the main trunk, but a large interosseous, which had arisen high in the arm. The next notice which I find of this peculiarity is by Dr Knox (*Ed. Med. and Surg. Journal*, 1841, p. 125), who—after reasonably doubting the correctness of a statement, which appears to have been made by Magendie, that there existed a "supra-condyloid" process and foramen in the humerus of the yellow races of Africa—states that approaches to the same structure might be found in "many" European arms; and records an instance in which the arrangement had just occurred to him in "the left arm of

¹ In many animals—amongst the *Quadrupeds*, *Rodentia*, *Edentata*, *Marsupialia*, and more especially the *Carnivora*—the median nerve and humeral artery pass through an opening in the humerus, which is formed by an arched process crossing obliquely the inner surface of the bone, close above the internal condyle.

a stout muscular middle-aged man," with the deviation of the artery and nerve in order that they might pass around the process. Another instance of this arrangement is recorded by Mr Quain in his recent work on the arteries, and is represented in Plate xxxvi. fig. 3, of his splendid illustrations of the usual and unusual distributions of these vessels.

I first saw this variety in the instance already quoted from Dr Knox, the preparation—showing the artery and nerve passing underneath the osseo-ligamentous arch—being now in my possession; and I have since met with it on seven occasions, in subjects dissected by me, or by the pupils under my superintendence. The first of these I met with four years ago, whilst engaged in practising the operations on the left arm of a female subject. The second and third occurred the following year in both arms of another female subject; and the remaining four during last winter session, being in both arms of two male subjects.

In all of them the protective arch was well formed, and transmitted the median nerve and undivided humeral artery, except on the right side of the last subject, on which the artery held its usual course, the nerve passing through the arch, along with a small muscular artery, which, in size and origin, resembled the inferior profunda. The osseous portion of the arch arose at a distance from the internal condyle, varying from an inch and a quarter to two inches and a half, and nearer to the anterior than the internal border of the humerus. It then curved downwards and inwards, and—in none of the above instances having exceeded three-fourths of an inch in length—gave attachment to the ligament, which, stretching in the same direction, joined the line close above the condyle, and thus completed the arch. In four of the above instances, the process, as now seen in the macerated bones, is from half to three-fourths of an inch in length. In the other three it is



shorter; more especially in that one in which the arch transmitted the nerve but not the artery, whilst on the other humerus, from the same subject, the process is the longest. The latter is represented in the accompanying outline of the lower third of the bone. The process and ligament are seen on their external and inferior aspect, so as to show the size of the opening as it now appears in the preparation.

I believe that this peculiarity of the humerus, when present on one side, will, from its nature, generally exist also on the other, unlike most varieties affecting at least the arteries; although in one of the subjects the difference in length between the processes was remarkable, as also was the difference in the position of the arteries. I may add, that I have on several occasions met with a similar want of correspondence in dissecting these parts in some of the animals in which the arrangement obtains; the bony arch being partly deficient on one side, whilst it was fully formed on the other. One of these was in a full-grown cat; in which animal also I have seen the opening on one side transmit—as in one of the instances above related of its occurrence in man—the nerve only; from which the natural inference may be legitimately drawn. I have also observed, in dissecting the young of these animals, that, at different stages of its development, the process resembles very much the conditions which it has presented in man. It is developed as part of the shaft of the bone, the ossification proceeding from above downwards; and very soon after birth it becomes united below to the lower end of the shaft, not to the epiphysis, of the bone.¹

The above instances of the occurrence of this singular variety in man, having led me to think that it might have been present more frequently than had been noticed or recorded by anatomists, I lately examined the preparations of these parts in the museum of the Royal College of Surgeons of this city; and amongst those which had belonged to Dr Barclay, I found four instances of it, although he has not alluded to the variety in his work on the arteries. Two of these occur in separate arms, both of the left side. One that of a very young child, where the process is long; the other that of an adult, where the ligament also is preserved. The other two are seen in

¹ This account of the formation of the opening in animals, is opposed to the view of those comparative anatomists who believe "that the passage of the artery through a foramen of the humerus, results simply from the extensive development of the inferior extremity of that bone," by which the vessel becomes at length encircled, in order, merely, that it may not be thrown out of its original and direct course; and tends to confirm the more common view, that the arrangement is one specially for the purpose of affording protection to the parts which the opening transmits; although, on the other hand, a consideration of the habits of the animals in which the arrangement occurs, and the dissection of the parts, do not render it evident that they, more than some others, require such a special means of protection for the nerve and artery. See "A General Introduction to the Natural History of Mammiferous Animals," &c. P. 89. By W. C. Linneus Martin.

the preparation of a child. The process is equally developed on both sides, but the ligament has been removed. On the right side, the undivided artery deviates and turns round the process. On the left, as in the other two instances, the radial artery arises above the middle of the arm, and continues downwards along the inner margin of the biceps, and the larger trunk (ulnar-interosseous) is removed to the inner side, and passes around the process.

To the fourteen instances above referred to or related, may be added, practically considered, four recorded by Mr Quain, in which the artery deviated in the same manner from its usual position. In two of these (op. cit. p. 259) the main trunk, in the others (as represented in Pl. xxxvi. fig. 4, and Pl. xxxvii. fig. 1) one of two trunks into which the humeral had divided, passed downwards along the inter-muscular septum, which they perforated or crossed at a distance of from two to three inches above the internal condyle.

The number of the above instances is sufficiently great to render the variety one of interest to the surgeon. It will be observed that in one-third of them there was a high division of the humeral artery, and that, whilst one of the trunks occupied the usual position of the vessel, the other was removed considerably to its inner side; and in the remaining two-thirds, that the undivided trunk was so situated, that it could scarcely have been reached by an incision made in the usual course of the artery. Further, at the bend of the elbow—as the pronator teres (or an additional muscular slip, which below joins the pronator) arises, in such a case, as high as from the ligamentous part of the arch—the artery is so overlapped or covered, that, before it could be exposed here, some muscular fibres would require to be displaced or divided; although, it may be remarked, the circumstances usually requiring an operation upon this part of the vessel could scarcely occur, owing to its deep situation.

It may be added, that the bony prominence was very readily felt before the dissection of the parts was begun, in the subjects in which it occurred in my rooms, even in the instance in which it was shortest; and the detection of this—in the preliminary examination, in a case where the operation was about to be performed—would, together with the situation of the pulsation of, and the effect of pressure on, the artery or arteries, at once indicate the existence of the variety in question to one whose attention had been called to its occasional occurrence.

Part Second.

REVIEWS.

Report of the Council of the National Institute of General Practitioners in Medicine, Surgery, and Midwifery, on the present state of the Medical Reform Question. 8vo. pp. 84. London, August 9, 1848.

WE are contented, for the present, to be chroniclers rather than critics, of the movements of medical reformers. This does not arise from indifference to the good work, but from apprehension lest in venturing on an opinion at the present time on points yet debated, we might possibly do injustice to some of the disputants. The facts on which our judgment must be based are not yet fully before the public; and we shall recur to the examination of them at a future time with the advantage of knowing the whole case. To the cause of medical reform we have ever been most ardently devoted; and we think that those who are lukewarm, and whose indifference assumes the form of supineness, when all the great interests connected with our profession are manifesting a just sense of the importance of reform, will deserve little sympathy if they should suffer for their neglect.

The cause itself is of great and undeniable importance. As respects professional men, it is the question, whether we are, in a medical sense, to continue to be three countries, or to be, in fact as well as in name, one united kingdom of Great Britain and Ireland; and whether or not we are to be subjected to vexatious iterations of the process of examination, serving no useful purpose, merely because we choose to shift our quarters from one part of the Queen's dominions to another. As respects medical incorporations, it is the question whether they will best consult their interests and their respectability by continuing to have a little preserve for each of them, and by prosecuting as poachers those who intrude upon it, or by exercising in common certain great functions for the benefit of the entire community of Great Britain and Ireland. And as respects the public, it is the question whether or not the present motley system tends to the good of her Majesty's lieges, and whether or not it be of importance to the people to have at all times access to the best and ablest advice, unembarrassed by restrictions of the kind we have alluded to. This is the question. It is, as we have said, a most important question. It is a question, the parties to which, in its threefold aspect, must have a common interest, if they are capable of seeing it; and it is not a little gratifying to observe the many indications now arising that they do see it, and are gradually preparing, in good faith, for a concurrent solution of its many difficulties.

An investigation of the medical institutions of the country before a committee of the House of Commons, commenced in the last Parliament, was resumed in the present. The evidence is now completed; a great deal of it has been made public by successive parliamentary reports; and the whole of it will soon be in our hands. During its progress professional men were made fully aware that they were expected by members of the legislature to facilitate their work, by endeavouring to co-operate on those great and broad principles of public utility which have so often received the sanction of large sections of the profession in every part of the country, and more especially by suggesting a plan for the legislative enforcement of a high and uniform standard of medical education and examination, as the basis of the desired equality of privilege throughout the whole extent of the British dominions.

In July 1847 a parliamentary volume came out, embracing the evidence taken, in the late Parliament, before the Select Committee of the House of Commons, "on medical registration and medical law amendment." The evidence proceeded entirely from witnesses on the part of the Royal College of Physicians of London, and of the Royal College of Surgeons of England, as there was no time for more before the dissolution of Parliament. On the assembling of the new Parliament in February 1848, a similar committee was almost immediately nominated, consisting, for the most part, of the same individuals, and having for its chairman the Lord Advocate of Scotland, instead of Mr Macaulay, who had filled that important office in the committee of the late Parliament. The new committee immediately proceeded to take evidence, which was ordered to be published on the 28th and 31st March 1848. It consisted of additional examinations of Fellows of the Royal College of Surgeons of England, members of the Council of that College; and of witnesses from the Apothecaries' Company of London, and from the National Institute of General Practitioners in Medicine, Surgery, and Midwifery.

While the evidence was thus proceeding in the Parliamentary committee, a most important movement took place in another quarter at the suggestion of the government. A conference was held in February, at the Hall of the Royal College of Physicians of London, between representatives of the two English Colleges, of the Society of Apothecaries, and of the general practitioners belonging to the National Institute which we have already mentioned, and whose very able and lucid "Report" is now before us. The result of the conference was the publication, in February last, of certain "Principles on which a Bill for regulating the Practice of Physic and Surgery should be founded," recommended by the joint authority of the parties we have enumerated. Of this document we gave some account in our April number (p. 765). It may be well to inform our readers that the National Institute professes to represent a very large body of the general practitioners of England, who

practise neither Physic, nor Surgery, nor Midwifery *exclusively*, but are engaged in mixed practice, consisting of the two former, or of all the three, and who are consequently inadmissible to the *fellowship* of the Royal Colleges, though many of them are *members* of the Royal College of Surgeons of England. The strictness with which the Colleges adhere to the principle of *pure* Physic and *pure* Surgery on the part of their Fellows, led those men to seek a separate incorporation as a Royal College of General Practitioners, which they had nearly attained in 1845, when Sir James Graham was at the Home Office. The failure of Sir James' proposed bill disappointed their expectations for a time. Some attempts have since been made to procure the incorporation of the general practitioners with the College of Surgeons of England; but they have been uniformly resisted by that college, and the conference is decidedly opposed to such attempts, its "principles" being based on the presumed necessity for the separate incorporation of the general practitioners as a third Royal College, and on the cessation of the functions of the Apothecaries' Company as a separate licensing body, in which, on that condition, the latter is willing to acquiesce.

The "principles" of the conference had thus received a large amount of support from the most influential bodies in the South, but had hitherto not been submitted to the medical men and medical institutions of Scotland and Ireland. As the session, however, advanced, the progress of the investigations conducted by the select committee of the House of Commons, made it necessary that representatives of the various medical interests of these divisions of the kingdom should be summoned to London to give evidence; and these gentlemen, as they came to town, were successively added to the conference at the Hall of the College of Physicians. Representatives were also admitted from the Universities of Oxford, Cambridge, and London. All this was done at the suggestion of the special committee of the House of Commons, to facilitate their labours, by suggesting such modifications of the "principles" of the conference as might be necessary, in order to accommodate these to the framework of their several institutions, and to the great and essential object of uniformity in education, examination, and privilege. These conferences appear to have accomplished in an important degree the object for which they were instituted. The representatives of large bodies of medical men in various parts of the country, pursuing the same general ends by somewhat different machinery, have been enabled by discussion to interchange important explanations, and to introduce such modifications of the "principles" previously agreed upon, and such additions to them, as were rendered necessary by the peculiarities of the institutions which they severally represented.

At the conclusion of the sittings of the conference (which lasted during the greater part of the parliamentary session), they were requested by the select committee of the House of Commons to present to them the result of their labours in the shape of the "outline

of a bill." This accordingly was done before the meetings of the conference were suspended; and the document contains, in a different form, nearly the same matter which we published in April.

The last portion of the evidence taken by the select committee was only very recently ordered to be printed. It will contain the examinations of the representatives of the medical institutions of Scotland and Ireland, and of the Universities of Oxford, Cambridge, and London. It will also contain the examinations of graduates of the London University, of *members* of the Royal College of Surgeons of England, and of extra-urban licentiates of the Royal College of Physicians of London.

In conclusion, we hope we may be permitted to congratulate our brethren of the medical profession on an increasing appearance of earnestness and good faith on the part of medical reformers, and on a more universal recognition by them of those great general principles of equality and equity upon which a reform of medical legislation must necessarily be founded, if it is to deserve that respectable title. We trust that in such circumstances the profession will be true to itself, and will be prepared to relinquish all narrow aims and exclusive interests which are opposed to the great general object. We borrow from the preface to the Report of the Council of the National Institute, the following excellent remarks on the temper and spirit in which the reform question ought to be approached by the members of the medical profession :—

"If the profession deem it just to the public, and expedient to itself, to listen to advice the direct tendency of which is to continue professional anarchy; if it deem it prudent to make demands which under present circumstances cannot be conscientiously conceded, either by the existing institutions, or by the Government itself; if it believe its influence sufficient to induce the Legislature to carry measures in opposition to the convictions of the parties now in authority; if it be conscientiously assured that an opposition to all reform is justifiable, because each section into which it is split, or each individual, cannot obtain the full complement which his own prejudices or his own aspirations have led him to desire; then let the present proposals be rejected. If, on the other hand, the profession can take a sober view of its own case, and, as a body of practical men, is willing to accept a real practical positive even though partial good, the advantage of which will be immediate, rather than aim at a shadow, and content itself with an Utopia which assuredly will never be realized,—then, in the opinion of the Council, it will cordially support the principles which have been assented to—ever remembering—that to secure the success of those principles, even under the present favourable circumstances, *unanimity* is most essential; that some of the more powerful parties to the arrangement may shrink from those active operations which are still necessary to carry it through;—ever remembering—that if, unhappily, the honest convictions of the active promoters of this measure are too lightly prized, and their efforts, founded upon those convictions, neutralized by any vexatious and irrational opposition, upon the heads of the opposing parties will rest the responsibility; and that, whatever evils themselves may have to bear with, from the indefinite postponement of all reform, or the enactment, amidst the chaos of opinion, of a measure inimical to their own interests or wishes, to none other than themselves can they impute the blame."

We trust, too, that the legislature will perform *its* duty with

firmness, and will feel it to be its high function to overrule those unreasonable objections and propositions which might otherwise again defeat the great object. Our legislators cannot now shelter themselves under the plea, that the subject has been insufficiently discussed, or that there does not exist a sufficient amount of concurrent opinion in the profession itself to enable them to do their part with effect; and they surely cannot think it wonderful that in a body of men so numerous, so separated from each other in place, and subjected to such various kinds of influences, there should still exist some differences of opinion on subordinate matters. Legislating for the medical profession would resemble no other sort of legislation if our legislators had merely to perform the easy task of putting the stamp of authority on that which has been prepared for them by others. They have now fully investigated the case; they cannot deny that the grievances are serious and their removal important; and we have a right to expect that they will perform their part as umpires with carefulness, firmness, and impartiality.—J. G.

Oratio ex Harveii Instituto in Aedibus Collegii Regalis Medicorum habita die Junii 24, 1848. A FRANCISCO HAWKINS, M.D., Coll. Reg. Med. Lond. Socio et Regestario. Olim Coll. Di. Jo. Bapt. Oxon. Socio. 4to. p. 27. Londini: 1848.

OUR readers are probably aware of the nature and occasion of the annual Harveian Oration in the Royal College of Physicians of London. The great discoverer of the circulation left to the College his patrimonial farm of about fifty acres, on condition of an annual social meeting being held, and a Latin oration delivered, exhorting the members "to study and search out the secrets of nature by way of experiment, and, for the honour of the profession, to continue mutually in love." We have often allowed the profane doubt to enter into our minds, as to whether the establishment of this oration has, on the whole, produced results commensurate with the benevolent aims of its distinguished founder. We are very sure, that could his modest and benign spirit have anticipated the small common-places of eulogium which have been lavished upon him from time to time; still more, could he have been aware of the manner in which it has of late been the practice to invoke his honoured memory, and the facts of his life, in favour of every crude theory which meets with a reception too cool for the glowing brain of its originator,—he would have been disposed to renounce posthumous fame for ever, seeing to what base uses it may be applied.

We prefix these remarks in order to have the pleasure of making an exception in favour of the oration before us. We have seldom read a character of Harvey at once so true, so pleasing, and so elegantly expressed, as that by Dr Hawkins in the following paragraph. It forms in itself a most admirable answer to those who, in the pre-

sent day, profane the memory of the great physiologist by making his name the signal for strife, and who attempt to bear down the scepticism of the world by fierce invective or querulous satire, instead of by the legitimate method of rational argument and experiment.

"Attendite, quæso, et cum animis vestris reputate, quisnam et qualis fuerit, qui nos *ad amorem mutuum* cohortandos curaverit? Is erat, qui, cum summum beneficium in omnes contulisset, tamen, a nonnullis, non sine contumelia atque convitio, oppugnaretur. Is, qui sanguinis motum circuitumque primus invenerat, ipse *Circulator* audiit. Et eum, nimirum, quem primo derisui habuerant, postea, insinulaverunt furti. At ille, qui 'nullius lacessivit famam,' ne suam quidam acriter defendit. Obtrektorum partim risit et neglexit, partim redarguit, at quam benigne! neque istos refellendi causa, sed quo pulcherrima sua reperta enucleatius explicaret. 'Convitia vero aut præterivit, aut laudibus rependit.' Testor Riolanum, 'hominem,' ut ait Laurentius, 'in disputando arrogantem et pertinacem,' quem, cum, 'erroris atque inscitie conviciasset, quasi unguentis delibutum atque floribus redimitum dimisit.'"

Dr Hawkins' address opens, as in duty bound, with a character of Harvey. He afterwards passes to an enumeration of the more distinguished members of the College of Physicians, to each of whom his meed of praise is awarded. The consideration of so many men distinguished alike in the practice of the healing art, and in general scientific and literary studies, suggests to him the errors of those who assert the uselessness of these studies to the medical man;—"qui *litterarum et scientiæ* studia medicis indici, saltem ex auctoritate, noluit; qui studia hæc medicis obfutura, atque ad medendum impedimento fore contendunt." He thinks that scrupulous and strict examinations and enforced attention to these subjects are necessary to the medical man, to preserve him from the rocks and shoals of popular applause and blame; "ne famam quærat et opes, non artis prudentia, sed verbosis strophis, atque stupore vulgi." He quotes from Lord Bacon to show, that medical capacity cannot be judged of by the public, like that of the advocate or the pilot of a ship, who are judged, he says, not by the issue of their acts, but by their inherent merit (?) Nay, in medical matters, the public are the more likely to be deceived, "quia magna pars hominum amat mirifica, et vult decipi: neque facile sibi errorem eum, quo delectatur, extorqueri patitur." Hence the numerous tribes of quacks, some of whom are very roughly, and we think ungenerously, handled by Dr Hawkins. It was scarcely worth while, and was stooping at very poor game, for a Fellow of the Royal College of Physicians to go out of his way to startle the ghost of St John Long, or to prefer such grave and revolting charges against the Mesmerists and Homœopathsists as we find here. We need not say that we have no extraordinary sympathy with these gentlemen; but we think they might be passed over, or reprobated in a more dignified and charitable style than that of the orator, without either the truth of history or the honour of the profession being less effectually vindicated thereby.

With respect to the remarks of Dr Hawkins on medical education and qualifications, we are far from participating the opinions of those against whom his arguments are directed; but neither can we agree with him in deprecating the influence of public opinion on the character of medical men. No doubt public, and still more individual, opinion may often err in estimating medical qualifications, still we think that there is generally a large share of justice in its awards; and as public opinion is, and always must be, the ultimate court of appeal by which professional reputations are judged, we have no doubt it will become daily more efficient, and will in the end be the only efficient bar to the practice of that charlatanism which Dr Hawkins so heartily abhors, and which, let us hint to him, may be found to almost as great an extent within the legitimate profession as out of it. Examination and enforced education are undoubtedly necessary for the security and honour of the profession, to prevent the admission of ignorant and incompetent persons into its ranks; but great care must be taken that artificial and arbitrary tests be not substituted in place of those natural and practical ones which the profession will readily acknowledge as just and proper, otherwise an oppressive pedantry, combined with real ignorance, will infallibly become the characteristic of the body of medical men, and will be met by contempt and disaffection on the part of the public. For the security of the public, the public itself must provide; and it is only when this feeling, hitherto so sedulously obstructed by medical corporations, becomes general, that men will proceed to choose their medical attendant with the same care, and deliberation, and reliance upon informed persons, as they employ in the case of their lawyer or banker. Then and then only, we believe, will quackery in its present aggravated forms cease to exist in this country.

After the extracts we have given, we need hardly say that this Harveian oration is excellent in its kind; it is the production of an active and refined mind, and is well adapted for its purpose.

The Latinity of this oration is unexceptionable. It is classical; it is smooth and flowing; it is free from pedantry. In freedom the style improves as the discourse proceeds. The great fault in the modern Latinity of men who are scholars, especially in set discourses like that before us, is a perpetual slavish reference to expressions in particular passages of classical authors. Dr Hawkins does not err deeply in this respect. But we are sure his style would not have suffered had he practised this habit even less. One of the great excellencies of the Latin language is the forcible simplicity of which it is susceptible. This beauty is seldom to be traced in modern Latinity. And the cause plainly is, that those who write Latin formally are commonly more desirous to show their familiarity with the writers in Latin than with the language. What a patchwork would an English discourse seem were it made up of expressions borrowed, in every second or third sentence, from the classics in the language? And yet this is the common plan on which much of the

modern Latin of scholars is written. Dr Hawkins has shown himself alive to the prevalence of this fault, though he has not been able wholly to avoid it. The practice of keeping up an annual Latin oration in the London College of Physicians, is undoubtedly a good one; and as it is fixed by Harvey's will, we are glad it is not in danger of being soon abolished by the indiscriminating fervour of modern innovation. We trust it may long continue to keep alive among the fellows of the College the laudable ambition of writing a pure Latinity like that of Dr Hawkins.

Part Third.

MEDICAL NEWS.

DEATH OF BERZELIUS.

THE public prints have already made our readers acquainted with the death of Berzelius, whose name has been for forty years a household word in the chemical laboratory, and is interwoven with the history of chemistry by a series of investigations probably unequalled, and certainly unsurpassed, either in extent or importance, by those of any of the great chemists of the present century. Prolific as that period has been in distinguished chemists, Berzelius has, perhaps, more distinctly than any other left the impress of his ideas upon the science; and, though the greatest step which chemistry has made belongs to Dalton, it is not too much to attribute to the Swedish philosopher a large share in the work of developing and establishing the atomic theory. For the few following details of his life we have been principally indebted to an article in the *Athenæum*, and partly to some other sources of information, which have enabled us to fill up some points slightly touched on in that article.

Jöns Jacob Berzelius was born in the village of Våfversunda, in the canonry of Linköping, in Ostgothland, on the 29th August 1779. His father was the village schoolmaster, and under his instruction he appears to have acquired his early education. At the age of seventeen he entered upon his medical studies at the University of Upsala, and here he also commenced his chemical studies. Afzelius was professor of chemistry at that time, but neither he nor his assistant, Ekeberg, though in other respects good chemists, appear to have excelled as teachers. The practical instructions especially appear to have been of a very unimportant character. The pupils were allowed to work one day in the week in the laboratory, where they were employed in trifling pharmaceutical operations. With these Berzelius was at first disgusted; but he soon came to frequent the laboratory daily, although he had no right to do so, and occupied himself in experimenting and observing in his own way. Although he was thus entirely thrown upon his own resources, there can be little doubt that he must have indirectly acquired much information from observing the researches which were going on in the laboratory; for Afzelius, though his health was bad, and he rarely published any thing, was constantly at work, and was in reality a very skilful chemist. It may not, perhaps, be generally known that he had discovered selenium, independently of Berzelius. After his death, his apparatus and specimens came into the hands of Berzelius, and among them there was found a small packet of impure selenium, which was dated, and marked "metal from Skrikerum," and which he had doubtless extracted from eukairite, which occurs there, and in which Berzelius proved its existence. In his early years, Berzelius had also the advantage of an intimate acquaintance with Gahn, who first applied the blowpipe to chemical experiments, and to whose instructions he owed the skill which he acquired in the use of that instrument.

In 1798 Berzelius passed his philosophical examinations, and in the following year became assistant to Dr Hedin, superintendent physician of the mineral waters of Medevi. His residence here gave occasion to his first contribution to chemistry, which consisted in an analysis of the waters of Medevi, which he published along with Ekeberg. In 1801 he received the license to practise, and in 1804 took the degree of M.D.

On leaving Upsala he came to Stockholm, where he was appointed assistant to Andrew Sparrmann, who accompanied Cook in one of his voyages round the world, and held the professorship of medicine, botany, and chemistry in the Carolinska Institut, the medical school of Stockholm. He at the same time practised medicine, but only to a limited extent; and as he devoted himself more completely to chemistry, he at length abandoned it entirely. On the death of Sparrmann, in 1806, he succeeded him in his professorship, in the duties of which he confined himself almost entirely to chemistry. In the same year he published, in conjunction with Hisinger, the first volume of his "*Afhandlingar i Fysik, Kemi, och Mineralogi*," a periodical work, which was continued at irregular intervals till the year 1818. The greater part of these volumes, six in number, was the work of Berzelius, Hisinger's share being comparatively small, and a few papers by other authors, forming too inconsiderable a proportion to require mention.

In the years 1808, a new era in chemistry was commenced by the publication of Dalton's new system of chemical philosophy. Berzelius was among the first to adopt the views of the English chemist, and from that time he devoted himself to its development, and commenced the series of researches on the atomic weights of the elements, which he may be said to have continued almost to the end of his life. In 1810 he was made president of the Swedish Academy of Sciences, and in his address on resigning the chair, he took a complete view of the state of animal chemistry, which was afterwards published, and has been translated into English. Berzelius had now reached a high degree of eminence, and his researches were considered of such importance, that the Academy of Sciences, in 1811, voted him an annual sum of 200 dollars, about L.16, to assist in defraying their expense.

In the year 1814, he published his "*Försök, at, genom användet af den elektrokemiska teorien och de kemiska proportionerna, grundlägga et rent vetenskapligt system för mineralogien*." In this important work, he pointed out that the atomic theory might be extended to all minerals; and showed that the greater number of them are true salts, in which silica is the electro-negative or acid ingredient. On these principles he deduced from his own analyses, and those of his predecessors, the true constitution of all the more common minerals; described a system of symbols by which they might be represented; and gave a sketch of a purely chemical arrangement of mineral species.

In the year 1818 he was appointed secretary to the Academy of Sciences, the highest scientific office which Sweden has to bestow, the duties of which he continued to perform till his death.

The number of cultivators of chemistry had now become so large, and facts accumulated with such rapidity, that the Academy of Sciences entrusted to Berzelius the duty of drawing up an annual report on the progress of physics, chemistry, and mineralogy. The first volume of this report was published in 1822, and it continued to embrace the whole of these branches of science up to the year 1839, when the physical portion was disjoined, and formed into a separate report, while Berzelius confined himself entirely to chemistry and mineralogy. The last volume which he published was that for 1847.

In 1831, Berzelius retired from the active duties of his professorship, and obtained the title of honorary professor. Up to this time he had remained a bachelor, but in 1838 he married the daughter of his old school-fellow, *Staats Rath Poppus*. At the same time he was made a baron, and the intelligence was communicated to him on his wedding-day, in a letter from the King himself. In 1843, having been for twenty-five years secretary of the Academy of

Sciences, a festival was given in his honour, the crown prince in the chair, and his portrait was presented to the academy. At this time, his health, which had hitherto been robust, began to give way; symptoms of an apoplectic character made their appearance, for which rest and change of scene were recommended. Berzelius accordingly spent the winter of the following year in Germany, and returned to Sweden in improved health. His constitution was, however, enfeebled, and early in last year he was seized with paraplegia. It was now obvious that his end was approaching, and he abandoned all interference with science. His mind, though perfectly entire, was incapable of prolonged exertion, and his last days were spent in hearing read to him works in polite literature, and in reviewing the occurrences of a well spent life. The natural vigour of his constitution enabled him to withstand, for a longer period than might have been expected, the ravages of his disease, and he died on the 7th of August last, in his 69th year.

Few men have embarked in the pursuit of science under more advantageous circumstances than Berzelius. The native of a country which has always been proud to honour, encourage, and reward her scientific sons, he commenced his career at the time when chemistry was rising up into a new science, and the recently established facts were beginning to be moulded into a philosophical form. He was fortunate also in bringing to its study a knowledge of the exact sciences; for, besides being well versed in physics, he possessed a greater knowledge of mathematics than usually falls to the lot of the general student, and the inductive spirit which these studies engendered, is apparent even in the very least of his works. To all these must be added an almost unequalled perseverance, which enabled him to pursue to a successful end investigations surrounded by difficulties which would have deterred most men.

Berzelius was a voluminous writer. Independently of those which we have already mentioned, he published, about 1806, a work on animal chemistry, in two volumes. His "*Lehrbuch der Chemie*," has gone through no less than eight editions. The earlier of these were in three or four volumes, and were published in Swedish and afterwards translated into German; but the later editions were translated from the manuscript of the author, and published in German, the Swedish circulation being too small to make so large a work remunerating. The seventh, which was completed in 1841, was in ten volumes; and the eighth, which was left unfinished by the author, and is to be concluded by Professor Wöhler of Göttingen, is expected to be still larger. Berzelius also published a work on the blowpipe, which has gone through five editions, and is admitted on all hands to be the most complete manual on the subject which has ever been published. Considering the extent of these works, one is surprised that Berzelius should have been able at the same time to pursue so many extended investigations, but he possessed a remarkable facility of composition. In illustration of which it may be mentioned, that his Annual Report, which is a closely printed volume of 500 or 600 pages, was the work of exactly a month. It was his habit to begin it every year on the first day of March, and it was presented to the Academy on the 31st, and during that month he never appeared to be particularly busy, but was as ready as at any other time to converse with his chance visitors.

Any detailed account of the numerous discoveries with which Berzelius has enriched chemistry, would extend this notice far beyond its assigned limits. He discovered the elements Selenium, Thorium, and Cerium. His researches on atomic weights have already been alluded to; and his investigations into the constitution of the compounds of the elements are too numerous to mention; and, in many instances, all our exact knowledge of these substances is derived from his experiments. But it was not merely to mineral chemistry that his researches were confined: his investigations of the different constituents of the animal body, are, in some respects, among the most remarkable of his works; for, though performed at a time when the whole subject of animal chemistry was in an almost embryonic state, there is scarcely a result which has not been

confirmed by more recent researches. Of this the discovery of lactic acid in the juice of flesh, is a remarkable instance. At the time when Berzelius made these experiments, the processes for preparing pure lactic acid were totally unknown, and its nature undetermined, yet, by a careful comparison of its characters with those of the acid existing in flesh, Berzelius was enabled to declare their identity; and although Liebig for some time denied the possibility of lactic acid occurring in the animal fluids, his latest researches have confirmed the old results of Berzelius. He also established the fundamental facts of vegetable chemistry by a series of analysis of the principal vegetable acids, &c., and afterwards pointed out the analogy in constitution between certain organic substances and the oxides of the metals, and on this he founded the doctrine of compound radicals, in the elucidation of which Liebig has acquired his well-merited reputation, and which, though now likely to undergo very great modifications, has been mainly conducive to the recent rapid progress of organic chemistry.

As an experimentalist, Berzelius stands unrivalled for his accuracy and precision. In all his researches he aimed at the highest degree of refinement which was possible, and on all occasions he avoided any thing like show. His laboratory consisted of two small rooms in the *entresol* of the Academy of Sciences, to which was attached a third, containing his library. Here every thing was arranged with the neatness which characterised all his operations. His apparatus was the model upon which that of all the other laboratories in Europe was formed, as indeed almost all the methods employed in the refined modern analyses were contrived by himself. He especially introduced the system of operating on the small scale, and to such an extent did he carry this, that he was enabled to complete the investigation of Thorium, including the determination of its atomic weight, and analyses of its salts, with not more than 60 or 80 grains of thorina. It is odd enough to contrast this with the discovery of Tellurium, which Muller von Reichenstein, being in some doubt as to its being actually a new substance, sent to Bergman, then the great chemical authority, requesting him to examine it. This he did, and soon returned for answer that he thought it was, but that the quantity sent was too small to admit of a positive statement. The *residue* of Bergman's experiments afterwards came into the hands of Berzelius, and is a piece about the size of a small pigeon's egg!

In the personal appearance of Berzelius there was little remarkable. He was short and stout, with the appearance of radiant health. The man of genius was not seen in his exterior, but there was an expression of *bon hommie* in his countenance which made a favourable impression upon a stranger, an impression which was invariably confirmed by a further acquaintance. His hospitality and kindness were great to all, but more especially to the cultivators of his own science, and no young chemist ever made his acquaintance without being delighted with his readiness to discuss the doctrines of the science, and to communicate the stores of his chemical experience.

His loss to chemistry is great, as he was the authority in whom all chemists had confidence. His position rendered him in a manner the great commentator on the progress of his science; and though his criticisms were in general temperate, no man could be more severe and sarcastic when he thought that any one had strayed beyond the bounds of careful induction.

DEATH OF DR WILLIAM CAMPBELL.

MANY of our readers will regret to hear of the death of Dr William Campbell, well known as an accoucheur and lecturer on midwifery in Edinburgh. He died on the morning of September 21, after a short illness, which latterly assumed an apoplectic form, most probably from ramollissement, or other disorganization of the brain. Ten days before, he had seemed to be in his usual health and vigour. Dr Campbell was a native of Argyleshire, and before settling in Edinburgh (now nearly thirty years ago), he had served for some years

in the medical department of the navy. During this period he visited the West Indies, and various other naval stations. Latterly he was a prisoner of war at Verdun; and he held a testimonial from Baron Larrey, granted for services rendered to the sick and wounded sent to the hospitals there from the wreck of the French army, after the retreat of Moscow. On the restoration of peace he took his degree in medicine at Edinburgh. In the winter of 1819-20, we believe, he gave his first course of midwifery, and from that time he continued to lecture on the same subject, with increasing reputation, down to the end of the summer session which has just closed. The museum which he collected for the illustration of his lectures stands almost unrivalled.

Dr Campbell was an author to no inconsiderable extent, and his productions show much ability, and an extensive acquaintance with the subject which he cultivated. In 1822 he published his "Treatise on Puerperal Fever;" in 1833 he published his "System of Midwifery," a second edition of which, much enlarged, he brought out in conjunction with his son in 1843; in 1842 he published his memoir on Extra-Uterine Gestation, a translation of which has since appeared in Germany. Besides these separate works he published a number of valuable papers in the Edinburgh Medical and Surgical Journal, the Lancet, and the Northern Journal of Medicine—of these the chief are, "On Hemorrhage after Instrumental Labour," *E. M. S. J.*, vol. xvi; "On Transposition of Abdominal Viscera," *ib.* vol. xvii; "Observations on Laceration of Perineum," *ib.* vol. xix; "On Laceration of Uterus and Vagina," *ib.* vol. xxix; "On the practice of accelerating Parturition in Deformed Pelves," *ib.* vol. xxxiii; "Case of Cæsarean Section," *ib.* vol. xxxviii; "On a new instrument, the *Kephalepsalis*," *Lancet*, vol. i., 1841-42; "Illustrations of Congenite Syphilis," *Northern Journal of Medicine*, May 1844; "Unusual Termination of a case of Congenite Hydrocephalus," *ib.* June 1844; "Cæsarean Section after the death of the Parent," *ib.* October 1844; "Statistics of 5754 Deliveries," *ib.* June 1845; "Report of the Obstetric Practice of the Royal Dispensary for 1845," *ib.* May 1846.

Dr Campbell was a man of indefatigable industry, and daily went through much exertion with little or no relaxation, and yet his health never appeared to suffer. He had hardly reached his sixtieth year.

PROGRESS OF CHOLERA.

From the 3d to the 4th of September, thirty-nine new cases of cholera occurred at Berlin; and from the 4th to the 5th there were sixty-four. The disease has also broken out at Spandau. The latest accounts from Constantinople, which are to the 25th ult., state that the cholera there continues much the same—from sixty to seventy cases per diem. At Adrianople its ravages have extended to 300 a day! At Trebizond it had diminished to ten or twelve cases daily. At Salonica it still rages to the extent of 1500 per week. Smyrna, too, is horribly afflicted: hundreds die daily; the dead literally bury the dead. Bazaars all shut, shops deserted, business suspended; more than 40,000 have left the city, which is now a prey to robbers. This is the description given of Smyrna by the last French steamer. The cholera has also appeared at Samsoun.

Germany.—The German papers contain returns of cases of cholera from the following places:—Berlin, the 8th instant; Stettin, the 5th; Magdeburg, the 6th; Vienna, the 8th; Moscow, the 26th of August; St Petersburg, the 1st of September; and Warsaw, the 5th. Up to the 8th instant there had been 784 cases of cholera in Berlin, 484 of which had terminated fatally; 205 patients are still under medical treatment, and the remaining 95 are reported as having favourably concluded. At Stettin, on the 6th, 32 new cases had occurred, and seven had died. The cases from the 8th of August to the 5th of September were 652, of which 433 had terminated fatally, 110 had recovered, and 109 remain ill. At Magdeburg, up to the 6th inst., there had been 79 cases, of which 35 had terminated fatally; from the 6th to the 7th, four new cases had

occurred, and five had died, making 83 cases, out of which 40 deaths; from the 7th to the 8th, 104 cases and 46 deaths; and from the 8th to the 9th, 17 new cases, and 10 deaths. At Vienna, the cholera had already broken out, and two deaths had occurred. At Moscow, on the 25th of August, there were 25 new cases and 16 deaths; on the 26th, 25 cases and 11 deaths. At St Petersburg, on the 1st inst., there were 53 new cases and 20 deaths. At Warsaw, from the 1st to the 4th of September, 214 cases, of which 53 cured, and 58 deaths. At Posen, also, the cholera had broken out.—*Examiner*.—More recent accounts state its arrival at Hamburg.—*Lancet*.

CHAP REMEDIES.

AN article with this title has appeared in a French medical journal, written by M. Recamier, ex-physician of the Hôtel Dieu ! The remedies proposed, and to which almost infallible powers are assigned, are cabbage poultices for gout and rheumatism, and cobweb pills as a substitute for quinine in intermittent fever. The rare sagacity and close observing powers of the author are eminently displayed in the discovery, that the cobwebs found in the houses of the poor are less efficacious than those which festoon the dwellings of the rich !

BOOKS RECEIVED.

1. Handbuch der Heilmittellehre. Von Dr Fr. Oesterlen, Professor der Medicin an der Universität Tübingen. Tübingen. 1845.
2. Lehrbuch der Arzneimittellehre. Von Dr. G. G. Mitscherlich. Erster band. Zweite verbesserte Auflage. Berlin. 1847.
3. Traité de Thérapeutique et de Matière Médicale, par A. Trousseau et H. Pidoux. 3me Edition, 3 vol. Paris. 1847.
4. Traité Philosophique et Expérimental de Matière Médicale et de Thérapeutique. Par G. A. Giacomini, Professeur de Médecine Clinique à l'Université de Padoue, &c. French translation by Moyon and Rognetta. Paris. 1839.
5. Etiological, Pathological, and Therapeutical Reflections on Asiatic Cholera. By A. Henriques, M.D., F.R.C.S.L. 8vo. London. 1848.
6. The Periodoscope : with its applications to Obstetric Calculations, &c. By W. Tyler Smith, M.B. 8vo. London. 1848.
7. Observations on Hospital Gangrene. By John Boggie, M.D. 8vo. London. 1848.
8. The Treasury of Natural History. By Samuel Maunder. 12mo. London. 1848.
9. Bulletin de L'Académie Royale de Médecine de Belgique. Tome VII. No. 7. Bruxelles. 1848.
10. Sanitary Questions, Observations, and Suggestions on the Wolverton Well Water. By George Corfe, M.D. 8vo.
11. Defects in the practice of Life Assurance, and Suggestions for their Remedy. 8vo. London. 1848.
12. A Treatise on the Advantages and Necessity of frequent Bathing. By Edward Perry. 8vo. Wolverhampton. 1848.

TO READERS AND CORRESPONDENTS.

Communications have been received from Dr WATERS, Dr DOUGLAS MACLAGAN, Dr THOMSON, and Dr BARKER.

Owing to the press of original matter in the present Number, we have been unable to insert the Reviews now ready on the works of Burnet, Morrison, Golding Bird, Matteucci, &c.

In the department of Medical News, we have been obliged to postpone articles on "Surgery in China," "Public Health Act," "The Hemlock Case of Poisoning," &c. They will appear in our next Number.

NOTICE TO SUBSCRIBERS.

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NOVEMBER, 1848.

No. 29. NEW SERIES.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I. — *Contributions to Toxicology. Cases of Poisoning, with Remarks.* By DOUGLAS MACLAGAN, M.D., F.R.S.E.

THE following cases are selected from among those in which, during the last few years, I have been consulted by the legal authorities. It is presumed that no apology is necessary for their publication. Even when they involve no very new or striking peculiarities, the details of medico-legal cases are important, as contributions to our store of experience.

Arsenic ; Imputed Poisoning ; Distinction of Arsenic from Antimony.

The last capital conviction in Scotland for poisoning with arsenic, was in the case of Thomas Leith, who was executed at Dundee last autumn, for the murder of his wife in April 1847.

The history of this case has been made known to the readers of the *Monthly Journal*, by a report of the trial in the Number for October 1847. As regards the poisoning of the murderer's victim, although the facts are merely those of an ordinary case of death from arsenic, it may not be superfluous to state a few of the details which are not given in the report in the *Journal*.

Leith, besides being conjugally unfaithful, had for some time pursued a system of cruelty towards his wife, one of the manifestations of which was, his depriving her of money, and thus curtailing her means of feeding herself and her children, from whom he was at the time of her death living separate.

About two months previous to her death, he endeavoured to fasten upon her the accusation of an attempt to poison him, by

NEW SERIES.—NO. XXIX. NOV. 1848. 2 N

mixing arsenic with his tea. His motive in imputing this crime was supposed by Mrs Leith herself to be to get her out of the way, by bringing her under the pains of law for the attempt, and thus to facilitate the adulterous intercourse which he was at the time carrying on with a servant girl; but it more probably was only a part of his deep laid scheme to poison her, for he more than once insinuated after this, that something would occur which would show that his wife was the real trafficker in poison, and not himself. He said to one witness, who had accused him of putting the poison himself in his tea, "Suspend your opinion for a short time, and something will soon take place regarding that woman, which will convince you beyond doubt that I have had no hand in the matter you charge me with, and that it will appear to you all that she has been the cause of all that has taken place."

On the fatal morning Mrs Leith was sorely pressed for money, and applied to her stepmother to procure for her a supply from her husband. Having received a small sum, she purchased some oatmeal and made porridge for herself and her children; but the quantity being insufficient, she had recourse to some barley-meal which had been for some time in the house, and she made it into porridge also. This barley-meal had been used for baking *scones* on a former occasion, and had been thought bad because it was sandy, but it produced at that time no effects resembling those of poison. Her motherly care seems to have led her to retain for her own use the porridge made of this inferior meal, and to give the newly purchased oatmeal to the children; and it was with this barley-meal, to which it was proved that Leith had access some days previously, that the arsenic had been mixed. She herself breakfasted on the barley porridge, but she left a little, and it was tasted by her four daughters. Her son, however, who came home from his work to breakfast, did not taste any of the barley-meal, but breakfasted on the oatmeal porridge alone.

The barley-meal was easily discovered to be the poisonous article; for, whilst the boy escaped who had eaten none of it, all the others who had partaken of it were more or less affected, and to an extent proportionate to the amount of it which they had eaten—Mrs Leith being worst, and her daughter Helen, who had eaten more than the others, being worse than any of her sisters, who escaped with comparatively slight symptoms.

The well-known observation of Christison, that no peculiar taste is in general perceived by the victims of arsenical poisoning, was amply confirmed in this case. Not only was no complaint on this point made at the time by any of the five individuals; but when the question was directly put in precognition to one of the daughters, she answered, that she did not "feel any ill taste that they (the porridge) had."

The interval that elapsed between the taking of the poison and commencement of the symptoms was not ascertained with pre-

cision; but it was amply within limits which make it a perfect example of that most important part of the general evidence of poisoning, the commencing immediately or soon after a meal. The impression on the minds of the sufferers was, that the symptoms had commenced in ten minutes, and they stated so to the medical attendants. But this statement can hardly be relied upon as fixing this time with precision. The girls, all under eleven years of age, might easily be in error as to the precise lapse of time, and the mother was not in a condition to be very clear in her statement to the doctors; for by the time they saw her she was "apparently moribund," "nearly speechless, and complaining of loss of sight, pain, and confusion of head." The interval was more probably longer, as the following narrative will show. Mrs Leith called on her stepmother Mrs Welsh at half-past seven A.M., and their interview does not appear to have lasted longer than was necessary for persuading the latter to call upon Leith and ask him to send some money to his family. Mrs Welsh's interview with Leith does not seem to have been of any great duration, and she returned straight from his shop to the house, and then found Mrs Leith supping the porridge. Even supposing each of these conversations to have lasted half an hour, which is certainly more than was the case, Mrs Leith must have eaten the porridge at little later than half-past eight. At a few minutes after nine her son Andrew came in for his breakfast, and at that time his sisters were out about the back of the house, and his mother was not making any complaint of illness. Before, however, the boy had begun to take his breakfast, which was waiting ready for him, his sisters came into the house, "spitting and complaining about the barley porridge, and some of them said they had been vomiting." To this Mrs Leith replied, that it was she who might spit and complain, as they had only tasted the porridge, meaning obviously by this, not that she felt any bad effects, but that she supposed that they were complaining of the quality of the meal, which they had tasted and condemned as being sandy three weeks previously. The boy remained at home till ten A.M., when he went to his work. His mother had begun to complain before his departure, but he saw nothing serious before he left, and his mother merely told him that she thought she was taking influenza.

The symptoms here manifested themselves earliest in the more irritable systems of the children, although they took the poison after their mother, and swallowed comparatively insignificant doses of it, one of them having taken no more than two spoonfuls. Previous repletion of the stomach seems to have had no effect in delaying the action of the poison. They who were first affected had immediately before taken their breakfast of oatmeal porridge; whilst the mother, who was the last to complain, had taken nothing but the poisonous food.

It appears then probable, in this instance, that the symptoms in the children had commenced in less than half an hour from the tak-

ing of the poison; that in the mother, the illness did not commence at all till at least an hour, and that no serious symptoms had manifested themselves when the boy left, about an hour and half after the time when she appears to have supped the porridge.

There was nothing in the subsequent progress of the case requiring recapitulation here. The children all recovered. Mrs Leith died at three P.M., retaining her mental faculties till within a short while of her death. No treatment was followed beyond washing out the stomach by the pump, as, by the guilty delay of Leith, the medical men were not called till she was in the hopeless condition mentioned above. At the period of their mother's death, the three eldest of the daughters were still vomiting, but nothing further of their history is on record after that.

Of the moral and general evidence bringing the proof of the crime home to Leith, I need say nothing here, as an excellent abstract of it has been given in the former report in this Journal. It may not be amiss, however, to give a little more detailed consideration to the attempt made by Leith to fix upon his wife the imputation of trying to poison him some weeks previously.

It was quite obvious, both from the general, moral, and medical evidence, that, to use the words of the reporter of the trial, "it is one of the established facts of the case, that he put the poison himself in the tea."

Leith, at the time of this imputed crime, lived in a great measure separate from his wife; but his victuals were cooked in his own house, and sent down to him to his shop, where he generally stayed. On the occasion in question, tea was sent to him in a flagon. It was prepared, as usual, by his wife, and sent to him by the hands of one of the children. He was seen by his shopboy to drink about a cupful of the tea when it was first brought to him; but immediately thereafter he ordered the boy to take down some articles which were hanging at the shop-door, and during this proceeding he carried the tea with him into the back shop. He thus did not lack opportunity of putting poison, if he had it, into the tea. About half an hour after this the shopboy's father called on Leith, and found him in his back shop. He was retching at the time, but this witness saw only marks of spitting on the floor, but no traces of vomiting. Leith said he had pain across his chest, but made no complaint of being sick, or of having pain in the stomach; and he did not actually vomit till he had chewed some tobacco and taken some warm water. His having tobacco in his possession at all was an unusual circumstance, for he was not in the habit of using it. To this witness he did not appear anxious or alarmed, nor did he receive the proposal to send for medical aid with that avidity which would have been manifested by a man who thought himself poisoned. He stated that he had taken nothing but the tea, and that if any thing had done him harm it must have been that. The shopboy's father then looked into the flagon, and saw "white ticks floating on the top,"

and, on pouring off the fluid, found a white stuff lying at the bottom.

It is well known to every one who has made solutions of arsenious acid, that a considerable amount of the fine particles floats for some time on the surface of the liquid, and does not subside until the fluid has been well agitated. It would be wrong to assume that it was impossible that any should remain floating, after the agitation to which the tea must have been subjected whilst it was being carried by the little girl from the house to the shop, had the poison been mixed with it previously; but the fact of so much remaining on the surface as to attract the immediate observation of the witness, may be taken as corroborative evidence that the arsenic had been recently put into the fluid; and it is remarkable that it should not have been observed by Leith himself, especially as his pretended belief, that his wife intended to do him some mischief, ought to have made him more than usually vigilant.

A medical man, Dr Lyall, was at last brought to Leith's assistance by the shopboy's father. To the doctor, Leith stated that he had pain in the stomach and bowels—most probably in reply to questions; for he had not complained of these symptoms previously, nor did he at any time manifest any symptom of diarrhoea or tenesmus. He stated that he had drunk about a cupful of the tea; that he had then gone to wait upon some customers, which detained him half an hour; and that then he began to be sick. Dr Lyall, on looking at the tea, found a white powder at the bottom, which afterwards proved to be between forty and fifty grains of white arsenic.

It must be observed that, even with all this quantity in the vessel, he might have, after all, had only a very small dose of the poison, from the sparing solubility of arsenious acid in such a fluid as tea—a cupful of which, at a temperature of 200° , in one experiment by Christison, did not dissolve two grains in half an hour. Still, had the arsenic been in the tea as he pretended, Leith must have taken enough to have caused more decided symptoms than those which he manifested, especially as the tea was purposely made weak, to suit his taste, and ought, therefore, to have dissolved more of the poison than usual. If the symptoms were not equal in degree to what might have been expected, neither were they in duration. The vomiting had ceased before Dr Lyall came, and, although he ordered an emetic, it does not seem to have been taken, for Leith vomited no more after that; and when the doctor called to inquire for him two hours afterwards, he was free from symptoms, described himself as being much better, and was speaking with his accused wife, who, on hearing of his illness, came down to inquire for him. This rapid disappearance of symptoms—sometimes a most important sign of feigned illness—affords a remarkable contrast, even allowing for differences due to age, to the subsequent cases of the children, one of whom, at least, had taken only two spoonfuls of the poisoned porridge, and yet continued very ill from nine A.M. to at least three P.M.

No one on reviewing these symptoms will hesitate to conclude, that Leith had not taken the poison which he pretended to have got, and that the vomiting, which formed after all the only palpable sign of illness which he presented, was the premeditated result of his chewing the unwonted tobacco, and subsequently swallowing warm water. The moral evidence, into which it is not my province to enter at length, strongly corroborated this view of the question. Instead of preserving the vomitings for the inspection of the doctor, Leith ordered them to be thrown out. Instead of being anxious for medical aid, he seemed indifferent to it, and was not so alarmed as his visitor, who found him sick in his shop. His wife, instead of preparing the tea in a secret or mysterious manner, took a portion of that which she and her children had used, added water to it, because he liked it weak, took down the flagon in presence of a female neighbour, looked into it to see that it was clean, and in doing so, held it, by chance, so near the other woman that she could see into the vessel, and immediately sent it away by the hands of her little daughter. That the latter could have put the poison in the tea was not insinuated. Leith, previous to the "tea story," was proved to be in possession of poison—his wife was not known to have had any dealings with such an article. She, instead of shunning her pretended victim, at once went to see him when she heard that he was ill. She, whilst she strongly protested her innocence, an assertion which she solemnly repeated to the clergyman when on her death-bed, and though distressed to tears by the accusation, yet urged him, in presence of witnesses, to find, if he could, proofs of her guilt. He, on the contrary, although he often repeated the accusation himself, showed great anxiety that the doctor, the police, and the neighbours, should make no further inquiries regarding it. Lastly, the respective characters of both parties operated most powerfully in forming public opinion regarding the transaction. No one seems for a moment to have thought that Mrs Leith was capable of such a nefarious attempt—every one, on the contrary, who spoke to him about it, seemed to jump to the conclusion that it was himself who put the poison in the tea. The doctor, it is true, knowing nothing of the previous history of Leith and his wife, seeing only a man complaining of symptoms of poisoning from taking tea prepared by his wife, and found to contain arsenic, was led to form, at first, an opinion against her; but this he soon gave up on seeing her composed demeanour. Perhaps the very greatness of the quantity found in the tea, may be taken as unfavourable to the supposition that Mrs Leith put in the poison; for, although murderers do not in general trouble themselves with any nice inquiries as to the solubility of toxic agents, it is but reasonable to suppose that, in putting poison into a fluid like tea, and into a limited supply of it which she might calculate on being all consumed, the murderer would have thought of observing whether such a quantity as forty or fifty grains—about the third part of a teaspoonful, according to Taylor—might not be

lying at the bottom undissolved. It is probably from the very fear of discovery in this way, that murderers generally employ thick fluids, such as gruel or soup, as vehicles for the administration of arsenic.

I now revert to the history of the fatal poisoning of Mrs Leith. I need say nothing of the morbid appearances, the essential details of which are given in the report of the trial in this Journal. They were quite correspondent with the supposed cause of death. The nature of the poison was also amply substantiated by chemical proof. Mr Hamilton, chemist at Dundee, assisted by Dr Nimmo, and in presence of Dr Crichton, detected arsenic, by all the usual methods, in the barley-meal, in the contents of the stomach *post mortem*, and in the matters removed from the stomach during life by the pump.

The various articles were subsequently sent to me for a corroborative analysis, without the results obtained at Dundee being made known to me, and I made the following report upon them to the authorities :—

"I certify that, on Thursday 29th April 1847, I received from Mr Wilson of the Crown Agent's Office a box, containing articles for chemical examination in the case of Thomas Leith, together with an inventory of the contents of the box. The box was opened in presence of Mr Wilson, and found to contain four bottles, a wide-mouthed glass jar, and two paper packets, corresponding with the inventory. The bottles, jar, and packets, were duly sealed; the seals were unbroken. The jar, on being opened, was found to contain the larger portion of the œsophagus, the stomach, and the greater part of the small intestines of a human adult. The mucous membrane of the stomach and bowels throughout, presented appearances indicating extensive inflammation. The surface of the mucous membrane of the stomach, which had previously been opened, had a tenacious mucus adhering to it, among which there were found minute crystalline grains, having the form and appearance of arsenious acid. The viscera were free of putrid smell. The jar likewise contained about three fluid ounces of a ropy bloody fluid, with a white mucous deposit at the bottom. This fluid, with the deposit, was examined chemically for arsenic. The process followed was that of Reinsch. [The process was here described in the report.] The copper, on being removed from the fluid, was covered with a steel grey crust. It was washed, dried, and heated in a glass tube, with free contact of the air. The heat produced a copious crystalline sublimate, the appearance of which, under a magnifying glass, sufficiently showed it to be white arsenic; but this was unequivocally established by dissolving the sublimate in distilled water, and subjecting the solution to the tests of the ammonio-nitrate of silver, ammonio-sulphate of copper, and sulphuretted hydrogen—all of which gave at once the reactions characteristic of arsenic.

"The presence of arsenic in the contents of the jar being so indubitably established, I deemed it unnecessary to make any experiments with the textures of the stomach and intestines.

"The bottle marked No. 1 in the inventory, and labelled as containing what was removed by the stomach pump from the stomach of Mrs Leith, was uncorked, and found to be full of a pale yellow slightly turbid fluid, with a feeble sour smell, and an acid reaction. Twelve fluid ounces of this were mixed with pure muriatic acid, and boiled with a piece of polished copper. The copper, after boiling, had a distinct grey crust, and when heated in a tube, as described above, gave also a crystalline sublimate of small amount, but sufficient, when dissolved in distilled water, to give all the characteristics of arsenic with the tests formerly mentioned.

"The paper packet, No. 6 of inventory (labelled as containing barley-meal found in Mrs Leith's house), was found to contain about half an ounce of a white powder, abounding in starch, and therefore apparently flour or barley-meal. About an eighth part of this powder was subjected to Reinsch's process, and gave a very copious crystalline sublimate, which, when dissolved in water and tested, gave the reactions characteristic of arsenic.

"I did not think it necessary, after the above results, to subject any of the other articles to experiment."

The other articles were merely more of the matters removed by the stomach pump, the washings of the tub in which the vomitings had been put before they were thrown out, and the bag in which the barley-meal had been contained.

The appearance of the stomach and intestines before any analysis was commenced, decidedly pointed towards arsenic, and led to that poison being at once specially sought for. The bright red inflamed appearance of the viscera, the absence of corrosion, softening, or blackening, and the complete absence of all appearance of decomposition after eight days of tolerably warm weather, indicated irritant poisoning, and the peculiar irritant to be arsenic. The antiseptic powers of arsenic are now thoroughly established, and constitute one of its most marked peculiarities. In this instance there was not only no positive putridity, but even that feeble but somewhat persistent cadaveric odour, which is familiar to every one who has dissected fresh bodies or organs, was wanting. As a general rule, where viscera with signs of inflammation are firm in texture, and in an unusually good state of preservation, arsenic may be suspected to be present, and to be in considerable quantity.

Before quitting the present instance of arsenical poisoning, I beg to offer a few remarks on the two most approved processes, those of Reinsch and Marsh, now in use for detecting arsenic in medico-legal analyses. The former may be said to be distinguished for its facility and celerity, the latter for its delicacy.

In any case, such, for example, as the present, where there is reason to expect a considerable proportion of arsenic, and where we have plenty of material to work upon, it is always best to have recourse at once to Reinsch's method, on account of its easy and rapid execution. As I have frequently found, not only in experiment, but in actual medico-legal investigations, the whole process, including the preparation of a tube for the experiment, may be satisfactorily done in less than half an hour. Where, however, the quantity of material at command is small, and the proportion of arsenic probably minute, the more delicate method of Marsh ought to be employed at once. Reinsch's method cannot be said to be remarkably delicate. I have failed to obtain any satisfactory evidence of the presence of arsenic when it was applied to a thousandth of a grain in two fluid ounces of liquid. Dr Christison says, that it will detect "at least a 250,000th part of arsenic in solution;" but this applies to the state of dilution, without reference to the quantity of arsenic present. Mr Taylor says it will act when a 3000th of a grain is used under a dilution of

90,000 times its weight of water ; but the action in his experiments appears to have been limited to the mere production of a stain on the copper. This is acknowledged to be no true characteristic. The bright copper is frequently tarnished when there is no arsenic present, and where all the necessary precautions in conducting the experiment have been attended to. The circumstances which occasionally lead to the formation of a stain very like that of arsenic, do not seem to be accurately determined. It will always almost occur in presence of organic matters, if the copper is put into the acidulated fluid before it is duly heated. Reinsch's process cannot be said to have acted satisfactorily for medico-legal purposes, unless it produces first a stain on the copper, and then a distinctly recognisable white sublimate. It is, however, a method capable of demonstrating very small quantities of arsenic. Mr Taylor detected the 144th of a grain in two fluid drachms of liquid. I have obtained all the indications of the presence of arsenic from 100th of a grain in two fluid ounces of thick soup.

The length of time during which the ebullition should be continued, has been, I think, rather overstated by both Christison and Taylor. The former says (4th edition, p. 272), "In the feeblest solutions, from ten to fifteen minutes elapse before arsenic is visibly deposited, and forty minutes should be allowed for complete deposition ; but in strong solutions the action takes place in a few seconds." Mr Taylor says (on Poisons, p. 353), "one caution is to be observed, *i. e.*, not to remove the copper from the liquid too soon. When the arsenic is in minute quantity, the deposit does not take place sometimes for a quarter of an hour." As the amount of arsenic meant by the terms "feeblest" and "minute," has not been specified, I may mention an experiment bearing on this point, made with a definite quantity of arsenic, not, however, probably so small as to bring it strictly within either of these categories.

One hundredth of a grain of arsenic in two fluid ounces of acidulated water, was boiled in the usual way with a piece of copper. After having been boiled for four minutes, the copper was found to have a slight grey crust, in eight minutes the crust was distinct, and in ten minutes the process was stopped. It was found, after this amount of boiling, not only that the copper afforded distinct evidence of arsenic, but that the whole had been removed, as none could be detected in the liquid by Marsh's method. A similar experiment, made with the same amount of arsenic in an equal quantity of thick broth, gave equally satisfactory results after ten minutes' boiling.

There does not appear, however, to be any practical disadvantage in prolonging the boiling. The mere coloration of the copper by the prolonged action of the acid, is, as Mr Taylor has accurately stated, not a source of fallacy, and the scaling off of deposited arsenic by prolonged ebullition, has appeared to me to occur only when the arsenic was in large quantity ; and in such a case the long boiling is not

required. In any case, where, after ten minutes' boiling, I find, on examining the copper, that it does not appear coated with arsenic, I remove the copper, boil the materials with the acid for some time longer, filter off the fluid, and transfer it to a Marsh's apparatus.

The peculiar form of the piece of copper, provided it be bright and clean, is not a matter to which I attach any importance. I give the preference to a piece of copper wire, of about the size called No. 24 by the dealers, made bright by rubbing it with a piece of sand-paper, and rolled into the form of a loose spiral coil, of about an inch long, by twisting it round a small pencil or glass rod. When of this form it is more easily caught and removed when it is immersed in a thick mixture of organic matters than a "slip of copper foil or wire" [Taylor], or "copper worn thin by the action of nitric acid" [Christison]. It affords, in a piece of moderate length, an extensive surface for the deposition of the arsenic, and thus expedites the process; and it is more readily washed, and seen to be free from adhering organic matters than copper gauze. In operating by Reinsch's method, I never make any preliminary filtration of the decoction, but boil the whole solids, if any be present, broken down as much as possible, with the acid, adding water if necessary, and at once immerse the wire coil in the mixture.

Marsh's process is, by universal consent, the most delicate method of ascertaining the presence of arsenic. It is no part of my object to comment upon the innumerable forms of apparatus which have been devised. By far the most convenient is the common Döbereiner's lamp, as figured by Christison.

The purity of the acid, sulphuric or muriatic, and of the zinc and water, having been duly ascertained, and the stop-cock and ground neck of the apparatus having been found by experiment to be airtight, so as to avoid any risk of loss of the gas, the suspected materials are to be boiled in water along with a portion of acid, and filtered through a piece of well-washed calico. It is then quite fit, in almost every instance, for being placed in the apparatus. The proportion of acid should not be great—the slower the gas is evolved the better. It is convenient to set the process going at the end of the day, and leave it all night. The apparatus is found full of gas, and ready for the decisive trial in the morning. The frothing up of the fluid, which has been such a bugbear to many experimenters, and has led to the troublesome and hazardous process of incineration, is productive of no practical inconvenience if this precaution is attended to.

The gas is now ready for being decomposed, so as to separate the arsenic. If the arsenic be in large quantity, it is of little consequence by what method the reduction of the arseniuretted hydrogen is accomplished. When the amount is minute it is best to adopt the method first proposed by Berzelius, of passing the gas slowly along a small glass tube, raised at one part by a spirit lamp to a low red heat, and thus obtaining a metallic ring, or more correctly incrustation, for it is seldom truly annular, in the interior of the tube.

In procuring stains on glass or porcelain, without considerable dexterity on the part of the operator, the quantity of arsenic, if very small, may be entirely lost; but in the use of the tube, if the gas is only passed along with moderate slowness, no loss can be experienced.

The most convenient tubes for this purpose are of German hard glass, about a twelfth of an inch in bore, and need not be more than five inches long. They are drawn out and turned up at the point, leaving in the apex a very small orifice, at which the disengaged hydrogen may be burned, and thus afford, by the size of the flame, a criterion of the rate at which the gas is passing. The reduction tube is connected with the nozzle of the apparatus by a portion of wider tube about two inches long, filled with dry cotton wadding, as recommended by Christison. The tube is to be heated at a little beyond its middle, till it appears feebly red when in shade. The tube, if of good glass, ought not to bend, and any tube which does so readily, or blackens when simply heated, from being made with lead, ought to be rejected. This method of operating, the simplification of which is due to Dr Christison, is so simple, easy, and sure, that all porcelain plates and watch glasses, and the ingenious tube devised by Dr Christison himself, for burning the gas and collecting the products of combustion, may be dispensed with.

It is however notorious, that the duty of the medico-legal analyst does not terminate with the procuring an incrustation on his tube or porcelain plate. What has been done is merely the elimination of something, the arsenical or non-arsenical nature of which remains to be determined.

Various matters have been indicated as being capable of producing deposits which may be mistaken for arsenical stains; but most medico-legal writers are now agreed, that antimony is the only one which offers any really practical source of fallacy, and hence a variety of methods have been described for distinguishing with rigorous precision antimonial from arsenical stains.

When the metallic stain is large this question is easily settled. Sublimation will in such a case yield enough of arsenious acid, if the deposit be arsenical, to afford precise characters by the liquid tests. But we may have to distinguish between arsenic and antimony, where the quantity would not furnish enough of solution for testing with two or three separate re-agents. The action of nitric or nitro-muriatic acid is that which has commonly been had recourse to for distinguishing stains of small extent. This, as is well known, converts the metallic arsenic into the very soluble arsenic acid, which gives a brick-red precipitate with nitrate of silver; whilst antimony so treated gives an insoluble product, and makes a grey stain with the silver test. But this proceeding is not easily applied, except to stains on a flat surface; and in procuring these there is the risk of a considerable or total loss of the arsenic, if the quantity be very minute, and the operator not very dexterous. The most complete me-

thod of distinguishing arsenic from antimony is that of Devergie [Ann. d'Hygiène, xxxvi. 121], which may be applied to stains either in a tube or on a flat surface, and has the advantage, that the whole series of reactions may be applied to a single stain without the necessity for dividing it, for the purposes of experiment, into separate portions. This method consists in, 1st, exposing the deposit to chlorine gas, which causes the arsenical stain to disappear by converting it into chloride of arsenic. 2d, The chloride so formed is exposed to sulphuretted hydrogen, which produces the pale yellow sulphuret of arsenic. 3d, This is treated with a very weak aqua ammonia, which makes with it a colourless solution; and this in its turn, being gently heated, reproduces the yellow sulphuret as the ammonia volatilizes. 4th, This yellow sulphuret is treated with a few drops of nitric, containing one drop of muriatic acid, and on evaporating this to dryness, white rings of arsenic acid are left, which, from their deliquescent under the moisture of the air, speedily become invisible. 5th, The spot moistened by the deliquescent arsenic acid is touched with nitrate of silver, which produces the dirty red stain of arseniate of silver.

This series of actions, if successfully evolved, is entirely conclusive for distinguishing a purely arsenical from a purely antimonial stain; but by M. Devergie's own showing, its success is a good deal dependent on the dexterity and nice manipulation of the operator. Devergie has, to a considerable extent, rebutted some of the objections which have been urged against his method. They are hardly valid where the stain is tolerably large; but he has given no positive statement as to the delicacy of his series of tests, or of how small a quantity of arsenic or antimony may be distinguished with certainty by its use.

There is one distinction between arsenic and antimony to which enough of attention has not been paid, and by which, I believe, they may be easily and readily discriminated, even in very minute quantity, viz. the difference of temperature at which they respectively undergo sublimation. That such a difference exists, is alluded to in general terms in most medico-legal works; but I am not aware that the comparative effects of a regulated temperature on the metallic stains has been adopted as a means of distinction, and adduced in evidence, except by Mr H. H. Watson, in the case of the *Queen v. Johnston*, Liverpool Lent Assizes, 1847. In the quotation of this evidence, as given by Mr Taylor in his appendix, Mr Watson says—"I also exposed some of these metallic deposits on glass, to a temperature ranging from 355 to 565 degrees, by which they were volatilized and left the glass. This is another proof that the metal is not antimony, but arsenic; antimony does not volatilize at the temperature mentioned, but remains permanent, while it is one of the properties of arsenic to become volatilized at that temperature."

It appears to me that, when this property is satisfactorily observed, there can be no possible mistake as to the stain being arsenical, and

not antimonial. Mr Watson does not state how he applied and regulated the heat; but I presume that it was by the oil bath. My experiments lead me to the conclusion, not only that it is the easiest and simplest way of distinguishing arsenic from antimony, but that it is a perfectly satisfactory method of separating them, and that they can thus be distinguished and separated in an unmistakeable manner, whether the stain be large or small.

The exact point of the thermometer at which metallic arsenic and arsenious acid sublime, is still *sub judice*. The arsenious acid is commonly stated to sublime at 380° , but, according to Dr Mitchell of Philadelphia, it requires a temperature of 425° . My own observations lead me to fix on 380° as the temperature at which, in a narrow tube, open at one end, arsenious acid begins to sublime. The metallic arsenic, commonly said to sublime at 356° , does not, according to Mitchell, volatilize except at a low red heat, luminous in the dark. I have not made special experiments for determining this point; but I have never been able to observe it sublime below 500° , unless it became oxidated. In a medico-legal point of view, however, this is not the present question. What we have to consider is, if we can, by a simple means, obtain a regulated heat at which metallic arsenic will sublime and become oxidated, whilst antimony will undergo no such change.

This we can easily accomplish by means of a bath of olive oil, which may be urged even to its boiling point without at all affecting an antimonial stain, whilst the heat so employed will entirely sublime an arsenical crust into a crystalline sublimate of arsenious acid. It will always be best, if it is possible, to have a thermometer in the oil bath, that the extreme temperature employed may be stated in evidence if asked for. But this is not indispensable; olive oil does not begin to boil till the heat rises above 600° ; and this heat, however long applied, does not cause antimony to volatilize. Stains which are so faint as not to appear distinctly metallic till the tube is held over a sheet of white paper, may be distinguished in this way. The pure arsenical metallic stain entirely disappears from the spot where it was deposited, the pure antimonial remains unchanged; whilst the mixed arsenical and antimonial becomes visibly less, a portion has undergone sublimation, and the residue, however long the heat may be prolonged, remains fixed. If, in addition to the disappearance of the stain from the portion of the tube immersed in the oil, we can observe the formation of a crystalline sublimate in the upper portion of the tube, the proof may be said to be absolute. Very small quantities of arsenic may be rendered distinctly visible in this form. I have operated upon stains produced from a Marsh's apparatus, which contained less than a thousandth of a grain of arsenic, and have yet been able to see distinctly the crystalline character of the sublimate. The gentle and gradual way in which the heat is applied in the oil bath, causes the sublimate to deposit itself in fewer but much larger crystals, and thus makes it much more appreciable

by the eye or lens than could be supposed by those who have been in the habit of subliming small stains of arsenic by a spirit lamp flame. In the case of some poisoned swine which I examined, and where, from one of the articles, I could obtain in the tube a mere shade of brown, and when, by the spirit lamp, this was sublimed into a mere white cloud, I was able, by again heating in the oil bath, to obtain a sublimate distinctly of crystalline appearance to the naked eye.

In order to test practically the value of this method of distinguishing arsenic from antimony, I made the following experiment, in which I was kindly assisted by my colleague, Dr Anderson. I requested him to prepare for me, in tubes, a series of stains from Marsh's apparatus, some of which should be arsenical, some antimonial, and others a mixture of both, and I proposed to distinguish these by the unaided operation of the oil bath.

Dr Anderson accordingly prepared for me six such tubes, which he duly numbered and noted.

No. 1 was accidentally mismanaged. Unreasonably trusting to my friend having taken the trouble to seal the tubes, I plunged this one into the oil with the point open, and it was of course filled with oil. Had such a misfortune occurred in a medico-legal investigation, it would have been easily rectified by immersing the tube in pure ether, which would dissolve out the oil; but as the crust in this case was large, probably the fiftieth of a grain, I felt so confident of success that I contented myself with merely blowing out the oil from the tube, then sealed up the point, and again placed it in the oil. The distinct crystalline sublimate, which speedily formed, showed at once that it was arsenical, and as it in time entirely volatilized, I pronounced against the presence of antimony.

Tube No. 2 presented a faint brown stain about two lines in length. Its brown metallic appearance was distinct only when it was held over white paper. It was pronounced to be arsenic alone, because it entirely disappeared, but it was so small as not to afford an unequivocally crystalline sublimate.

No. 3, on being heated for about ten minutes, presented at the upper part of the tube a faint crystalline sublimate, but a large metallic stain remained below. It was subjected to a prolonged heating, and no more sublimation could be observed. It was pronounced to be antimony with a small proportion of arsenic.

No. 4 gave a copious distinctly crystalline sublimate in a few minutes, and the whole stain eventually sublimed. It was recognised as a large arsenical stain, as in No. 1.

No. 5. The stain in this tube was so faint as to appear greyish-white when held to the light, but it was observed to have a faint brown tint when held over white paper. It was heated for a length of time, but underwent no apparent diminution. It was pronounced to be a trace of antimony.

No. 6, a large stain was not affected by heating for above half an hour. It was evidently a large antimonial stain.

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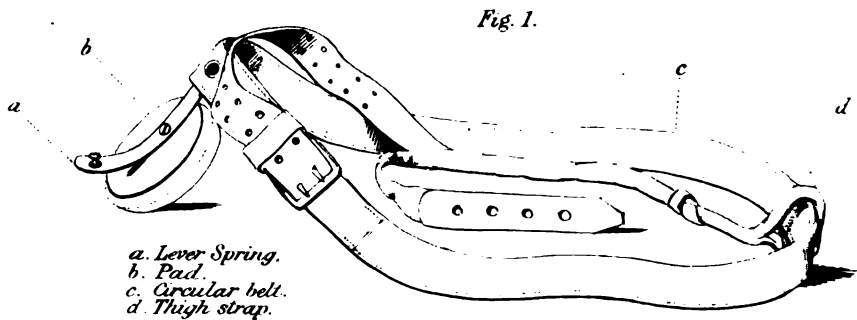
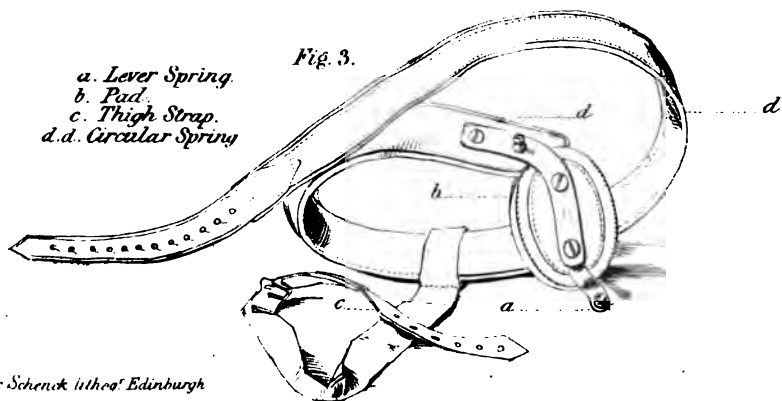
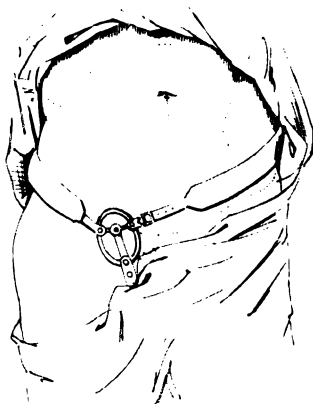


Fig. 2.
Evans's Mor-main lever truss applied.



Fr. Schenck lithog. Edinburgh

ILLUSTRATIONS OF DR THOMSON'S PAPER ON VARICOCELE.

to face page 295.

In each of these instances I found that I had pronounced correctly. The minute quantity distinguishable by these simple means, may be inferred from this, that, in No. 5, the Döbereiner's lamp contained only three-thousandths of a grain of tartar emetic, and as this salt contains only 35·9 per cent. of metallic antimony, there could not have been in the tube more than a-thousandth of a grain of metallic antimony. But as only one charge of gas was used, it is most probable, from the faintness of the stain, that only a portion of this was collected. In order, therefore, to determine whether a stain be arsenical or antimonial, all that is required is to operate with Marsh's apparatus and the narrow glass tube; if a stain is procured, to seal up the point, immerse it in the oil bath, and heat this steadily. The heating does not require to be prolonged. Ten minutes after the temperature has risen to about 500, will have affected the stain if it is arsenical. It is well remarked by Taylor, that, in such investigations, we have to determine the presence of arsenic in antimony, not of antimony in arsenic; and therefore, if any sublimation in the oil bath can be observed at all, the question as to the presence of arsenic is settled. To enable us to observe this more readily, it is a good plan to make a small scratch on the tube at each limit of the stained portion before heating it, and thus, by its diminution, we may pronounce upon its nature, although no sublimate should be distinctly visible. Should any peculiar case occur, in which it might be of importance to determine that antimony was present, as well as arsenic, the heat must be continued for a longer period. I have found that a large pure arsenical stain, weighing on a delicate balance 0·036 grain, required an hour and a half of heating at 500 in a narrow tube to sublime it entirely. But long before one-third had been sublimed, the tube was lined with splendid crystals of arsenious acid. The heat, to sublime the whole arsenic, need never be raised beyond 520, but, even if the oil boils, it does not affect the correctness of the experiment.

I believe, therefore, from what I have been able to observe, that the nature of a metallic stain may, in this way, be accurately determined without the employment of any chemical re-agent whatever.—
(*To be continued.*)

ART. II.—*Varicocele treated by Pressure.* By LAWRENCE RAMSAY THOMSON, M.D., Edin., Dalkeith.

THE following pages have been written with the view of inducing surgeons to try a plan of treatment for the cure of varicocele, the nature and success of which seem little known to the profession in general. That it is little known, I am led to believe from conversations with our principal surgeons in Edinburgh on the subject; and from the fact that, in the medical journals of the day, little mention is made of this safe and successful method of cure. On the other hand, scarcely a periodical appears that has not a description of one or other

of the numerous and hazardous operations that are continually practised for the radical removal of this complaint.

Mr Curling of London, so far as I am aware, is the only one who has published cases of varicocele cured by this plan : and here I gladly seize the opportunity of acknowledging the benefit I have derived from what he has written on the subject.¹

In the monthly *Retrospect* of this Journal for August last, there is given a case of cure by M. Vidal's process, which, with the exception of M. Breschet's plan, is perhaps the most generally successful of the seven or eight different methods which have been resorted to for the obliteration of the spermatic veins, when affected by this disease. But we not unfrequently hear of disastrous consequences following even this operation. A few days ago, Mr Syme mentioned to me that he had seen lately, in London, a melancholy case of fatal phlebitis following an operation for obliteration of these veins.

In having recourse to any means for the relief or cure of varicocele, it ought always to be borne in mind that the affection itself is not at all a serious one ; and, therefore, that no surgeon is justified in having recourse to any dangerous expedient for the removal of a disease only troublesome, not dangerous in its effects. If warranted in any extreme case—as when much pain is experienced, and when injury to the testes may be threatened—an operation ought only to be had recourse to when palliative measures and the method I am about to explain have failed in giving relief. Mr Syme, and other judicious surgeons who have not yet tried this plan, seldom or never now have recourse to such dangerous procedures as those of Velpeau, Breschet, Ricord, &c.—thinking it better to be content with palliative treatment than to risk the lives of their patients, by performing operations fraught with much danger. This palliative treatment consists of frequent bathing of the parts in cold water, together with the use of a suspensory bandage, or, in urgent cases, excision of a portion of the scrotum, to serve the same end more completely, and with less trouble to the patient. The curative measure we now propose is not more troublesome than an ordinary suspensory bandage, and certainly greatly to be preferred to the latter of these modes of trussing. We hope the day is not far distant when the details of such doubtful and hazardous operations as those of Velpeau, &c., will only be interesting as matters of surgical history. As a means of accomplishing this desirable end, we confidently look forward to the *pressure* plan of treatment doing much.

The plan appears *a priori* opposed to the intentions to be fulfilled in the treatment of varicose veins in general. I hope, however, to be able to show in the observations to be made on the following case, that the plan is not only sound in practice, but intelligible in theory.

¹ "Nor am I aware of any one else having written on this subject." Mr Curling's letter, hereafter quoted, August 22, 1848.

In several instances I have recommended this practice to other practitioners; but only in a few instances, as yet, have I personally superintended this method of treating varicocele. The following is the only case that has been under treatment sufficiently long to render it worthy of detail.

CASE.—Large Varicocele on the Right Side, upwards of Twenty Years' standing, completely relieved by Pressure.—This varicocele, which consisted of a plexus of dilated veins wholly surrounding the body of the testicle, and extending up the inguinal canal, gave rise for many years to great uneasiness, consisting principally of a disagreeable sense of dragging and weight from the loins, and occasional sickness after much exertion. Standing long in one position gave rise to the same; and sometimes to a dull aching pain in the affected parts. The testicle of the affected side was not smaller, as often happens in such cases, but felt somewhat softer than that on the sound side. Two surgeons, who saw the case about four years ago, were of opinion that it was one of scrotal hernia; and accordingly recommended a spring truss to be applied. This was done for a time, but could not be persevered with, as the ordinary hernial truss did not prevent the tumour re-appearing—when pain was produced to such an extent as to require immediate removal of the instrument. About this time Professor Syme was consulted, who soon detected the real state of matters. He immediately forbade the use of the truss, and ordered the local bathing with cold water once or twice a-day—and that the trowsers of the patient should be worn well *braced up*. By this means, the trowsers being worn so as to exert not only pressure over the scrotum, but considerable pressure on the dilated veins at the external ring, great relief was given to local and general, including mental, uneasiness.

As is common in cases of varicocele and other secret disorders in early life, there was great mental distress in this case—out of all proportion to the actual disease. At times the depression of spirits had been such as to interfere with all mental exertion of any useful description. Much of the relief afforded in this last respect, I have no doubt, was due to the assurance given, that the affection was not a serious one—at the worst only an inconvenience. But that the relief mainly depended on the pressure exerted in the manner described, was proved by the circumstance, that when trowsers were used that did not admit of this *bracing*, the pain and many of the other symptoms immediately returned. Here it may be stated, that no sensible benefit had been derived from the use of the ordinary suspensory bandage. It was my knowledge of the effects of the bracing on the dilated veins, that led me to recommend for this case a simple contrivance, that might exert the necessary pressure independently of the trowsers. This bandage (see Fig. 1, of Plate) consisted of one strap or belt to encircle the abdomen, with a pad projecting from the right side of it, to be retained over the tumour in the inguinal canal, and the pressure there regulated by means of a thigh-strap. This afforded relief, but still was not sufficient to remove the disagreeable sensations already described.

Some time after this I was much gratified to find, on reading Mr Curling's valuable work "On the Testis," that he had recorded there two cases of varicocele, one of which had been completely relieved by pressure—exerted by Evans's patent lever truss—in twelve months, and the other, as subsequent observations¹ have shown, completely cured in fifteen months. Although opposed to preconceived notions of the indications to be fulfilled in the treatment of varicose veins, yet from what I had seen of the benefits of pressure, as already stated, I had no hesitation in giving Evans's truss a fair trial. Before applying it, Mr. Curling was consulted, when, on examining the case, he

¹ London Med. Chirurg. Transactions, vol. xxix.
NEW SERIES.—NO. XXIX. NOV. 1848.

at once agreed with me in thinking it one of decided varicocele, and, moreover, very favourable for treatment by pressure. Accordingly, Evans's patent (moc-main) lever truss (Fig. 2) was applied over the external ring. The patient, as the result of this trial, was not more astonished than delighted to find that, for the first time in his own recollection, no tumour appeared in the scrotum on rising from the recumbent posture. Now and then, however, the tumour reappeared when violent exercise was taken on horseback, or otherwise. From this circumstance, and other reasons that will be mentioned in the subsequent observations, if space permit, I was led to invent a more efficient truss for this case of old standing varicocele. This instrument (Fig. 3) consists of a small circular spring, with a pad attached to it, so constructed that it can be made to extend as little and as much beyond the external ring as may be thought necessary for the extent of the affection, or as the shape of the pelvis may require. On the outer surface of the pad is fixed the same simple lever spring that I had made for the first truss, and is nearly the same as that on the moc-main truss—which I would recommend only for slight and recent cases of varicocele. To the free end of this spring is attached the ordinary thigh-strap, by which, while steady pressure is kept up over the inguinal canal, the pressure can be increased or diminished at will over the external ring. Since the employment of this last instrument, now ten months ago, though most violent exercise has been taken, preternatural dilatation of the veins has not once occurred; and, during the whole of that period, perfect immunity has been experienced from all the distressing annoyances of a varicocele. The patient is now apparently free from the disease, and both testicles feel alike natural.

This case is interesting in several particulars. The varicocele was on the right side, had been of long standing, had occurred long before puberty, and had been mistaken for a hernia by two experienced surgeons. Before touching on any of these points, it will be as well here to explain as far as possible the *modus operandi* of the *pressure* plan. As already remarked, it is opposed to former belief; namely, that all pressure exerted on a vein, short of causing obliteration between a varicose enlargement and the heart, must increase, not diminish such enlargement.

I am convinced that many surgeons have been deterred from trying the plan I am now advocating, from the erroneous impression that the force applied must be such as to cause obliteration of the veins. Professor Miller, in his treatise on practical surgery, mentions the possibility of applying such force by means of a spring truss; but, at the same time, informs his readers that it is the "most objectionable of the modes of cure."¹ Fortunately, the plan I am advocating does not aim at obliterative pressure—it only seeks to restore the diseased vessels to their natural tone and calibre. This is to be effected by maintaining such an amount of firm, steady, and equable pressure over the external abdominal ring as shall not permanently obstruct, merely afford the weakened vessels proper support at that

¹ Mr Miller recommended in the case given in the text *gentle* pressure, to be applied by means of a spring truss, not as a curative but palliative measure, and mainly to prevent herniary protrusion. This last we think an important end to be gained by the use of the truss in cirsocele; for we cannot help thinking, as stated by Mr M., that varicose veins extending up the inguinal canal may pave the way for hernial descent.

point, so as to remove the superincumbent weight of the blood from the distended veins below. That the pressure shall temporarily supply the place of valves, rendered incompetent from over-distension, is the important desideratum. When the vessels shall have regained their wonted strength and size, then the valves will resume their proper function of resisting hydrostatic pressure. After this end is gained, a cure is established. When properly applied, there is not the slightest risk of the pressure acting injuriously on the testicle, by interfering with the circulation of the spermatic artery. So far from such a result occurring, the testicle will resume its natural size under the influence of pressure, though formerly atrophied on account of the affection. The venous return will be carried on by the vessels not affected with the disease, and probably also to a certain extent by some of the diseased vessels themselves. We are aware that some difficulties in relation to the forces moving the blood, and the pressure exerted by that fluid on the vessels, are presented to the clear comprehension of this theory. And we would here observe, that we rest the claims of this mode of treatment on its practical results, and not on the theory advanced to explain it.

It will be seen from the foregoing statements that a nice adjustment of pressure is necessary. The instrument used by Mr Curling for this purpose is what has been called the *moc-main*¹ lever, or Evans's patent truss. This instrument, as will be seen from the drawing, has only a lever spring. Though devoid of a circular spring, I believe it to be sufficient for the cure of most cases of cirsocele, but not without causing great discomfort, when much pressure is necessary to support the superincumbent weight of the blood. In cases of old standing, such as the one given in this communication, it may even prove unequal to this task. The discomfort does not arise from the pressure exerted by the pad, for it seems superior to others in this respect (see foot-note), but from the tightness with which it is necessary for the encircling belt to be drawn when much pressure is wanted. The *moc-main* truss being faulty in these two particulars, i. e., not always able to keep up the varicocele, and giving rise to uneasiness from the tightness with which it is sometimes necessary to fasten the encircling belt—I was led to contrive a different and more efficient kind of instrument. This is a combination of the ordinary spring and lever truss; it has therefore a circular and lever spring, to the former of which the pad is attached (as described at p. 298) in such a manner as to admit of slight elongation. The pad may or may not be stuffed with *moc-main*; the simple cork and

¹ The truss is so called after the name of the material with which the pad is stuffed. "*Moc-main*" is the produce of the silk cotton-tree, *Bombax heptaphyllum*, a plant which grows abundantly in the East Indies. It is of a white, shining appearance, and possesses great lightness and elasticity, on account of which the pad stuffed with it "allows," as Mr Curling has stated, "of the application of greater force than could be tolerated under other circumstances."

flannel pad will be easily borne in most cases. The length of the pad should depend upon the extent of the disease. Some months ago I was consulted by a person labouring under a varicocele of considerable size, accompanied with oblique inguinal hernia. In this case I caused the pad to be made a little longer than usual, so as to press firmly over the internal as well as external ring; and also, to have the encircling spring made stronger than usual in the truss I advise for varicocele. Cases of this kind are continually neglected, from ignorance of the plan of cure I am recommending, inasmuch as for their relief only a palliative measure is generally prescribed. Very lately I was told of a case of large varicocele, complicated with reducible hernia, where the patient suffered much, and where the surgeons consulted advised only a suspensory bandage, thinking that the condition of matters would only be aggravated by the application of a spring truss. I need scarcely say, that when the case was mentioned to me, I recommended the person should procure a truss with such a pad as I used in the case just alluded to. In the formation of the pad, care must be taken that it be not made too conical; for, if so, the veins may be separated instead of supported. Between the skin and the pad there should always be interposed a piece of chamois leather, which can be changed as often as necessary. We here recommend, as an important adjunct, that the trousers be worn so as to take off the weight of the testicles from the cord. The thin circular spring (not required so strong as in hernia) of this truss, if well fitted to the body, must be much more agreeable than a belt tightly encircling the pelvis. But the instrument I have contrived has other important advantages over that hitherto used by Mr Curling. From the oblique direction of the ends of the encircling belt of that used by Mr. C., when attached to the pad and on the body, there is a continual striving for displacement of the pad upwards, which, to a certain extent, does away with *direct* pressure over the spermatic veins; also, if the thigh-strap of the moc-main truss become disengaged (as often happens), the whole pressure is instantly removed. In the truss I recommend for varicocele, the responsibility of the pressure is divided between the circular and lever springs; so that, even if the latter should become temporarily useless from disengagement of the thigh-strap, the pressure of the former is still sufficient for a time to prevent distension of the veins. Again, the moc-main truss requires the thigh-strap to be always more or less tight, for upon it its usefulness entirely depends; whereas the thigh-strap in my truss need not always be tight,—in fact, may occasionally be dispensed with altogether. Pressure is kept up over the important point (the external ring), to a certain extent, without the thigh-strap. It is only, therefore, when much and violent exercise is taken, that it need be tightly applied.

Diagnosis.—A knowledge of the *pressure* plan of treatment is highly important in the diagnosis of varicocele. Ignorance of it caused the above case to be mistaken for scrotal hernia. The

surgeon who did so stated, when consulted,—“You see that the tumour disappears on the patient assuming the recumbent posture, but does not reappear, while my fingers remain firmly pressed over the abdominal outlet, on the patient rising to the erect posture. If it had been a varicocele,” said he, “it would have reappeared; being a hernia it does not.” The same doctrine is very generally taught in systematic works. To show this we make no selection; we only quote from the works that happen to be on our table. “Pressure is then made at the ring and the patient rises, when the swelling will reappear if depending upon vascular enlargement, as the blood cannot thus be prevented from finding its way through the arteries; but the tumour will not return if of a hernial nature.”¹ “Unlike hernia, there is return of swelling on resumption of the erect posture and on abdominal exertion, though the thumb be kept accurately and firmly placed on the abdominal outlet.”² Although the first quotation does not say positively, like the second, that the pressure must be *firm*, yet it is sufficiently calculated, in many instances, to mislead. Mr Syme himself admits, that the test, as given in the above quotation, is “frequently ambiguous;” he was therefore too much the practical surgeon to trust to it in the above case. He had met with a few cases where, by pressure, he said, it was quite possible to keep up a varicocele for a short time, and he looked upon this case as one of the same kind. Had the effects of pressure, exerted by a truss such as I have recommended, been tried in these cases, it would have been found that the veins might have been kept *permanently* free from morbid dilatation. It is in cases of this kind—namely, in those where the veins are found to be relieved by the firm application of the fingers to the external ring—that the treatment is most applicable. Indeed, we feel convinced that, in all cases, other things being equal, in which firm pressure exerted by the fingers is sufficient temporarily to keep up a varicocele, a cure is to be regarded as certain when adequate pressure is applied permanently by means of a truss. The case I have given goes to prove this, as do those published by Mr Curling.

It is very frequently stated in books, that the diagnosis between varicocele and hernia is an easy matter. The case detailed proves the contrary. To make a correct diagnosis is of great consequence to the patient, as the application of an ordinary oblique hernial truss, where the principal pressure is wanted over the internal, and not, as in the case of varicocele, over the external ring, may seriously inconvenience the patient, and aggravate the disease. We have the testimony of Mr Syme as to the difficulty of diagnosis when the dilatation occurs high in the cord, in which case the dilated veins receive an impulse on coughing, and in other respects resemble an inguinal hernia, particularly one consisting of omentum. As the

¹ Syme's Principles of Surgery. 1837. P. 362.

² Miller's Practice of Surgery, p. 366.

most certain means of ascertaining the truth, the same author directs the surgeon "to compress the neck of the swelling while the patient stands erect, when, if composed of dilated veins, *it will become more tense*." There is a statement made by Mr Curling,¹ which would show that this plan may also prove fallacious. He says, "In a patient affected with this disease, if the spermatic cord be pretty firmly compressed between the fingers whilst the patient is in the recumbent position, and the vessels are empty, it will be found, on his assuming the erect position, that the vessels, instead of swelling as before, still remain empty and contracted. Even, too, when the patient is standing and the veins are full, if firm pressure be made on the cord, the vessels below, being thus relieved of the superincumbent weight of the blood, will gradually become emptied of their contents."

Notwithstanding this latter statement—for the truth of which in every case, if in any, we would not vouch—the plan of grasping the cord firmly between the fingers—pinching and rubbing the vessels between finger and thumb—is, in the hands of the experienced surgeon, perhaps the most certain means of diagnosis; not because in this way the tumour becomes more tense (which it may or may not, according to the amount of pressure exerted), but because educated fingers can best by this means distinguish the tortuous and dilated vessels of a varicocele from other tumours of these parts. The student may still be taught the ordinary plan, as given in books, provided he be made aware that the pressure must be *gentle* and not *firm*, as most authors have it; for, as has been shown, if the pressure exerted be great, the veins may remain empty and contracted. Gentle pressure in most cases will suffice to keep up a hernia, but not a varicocele: firm pressure in most cases will keep up both. I would recommend the following manner of stating this method of distinguishing between the two diseases. Place the patient recumbent, and raise the scrotum until the swelling disappear: let gentle pressure be now applied at the abdominal outlet, and the patient made to assume the erect posture, when, if a varicocele, the tumour will reappear, but not so if a hernia. The tumour will be seen to reappear from below if a varicocele, and, on all pressure being removed, from above if a hernia.

In concluding these remarks, we would repeat the great principle of treatment to be, the application of such an amount of pressure at the external ring as shall take off the weight of the column of blood from the distended veins below, and, in this manner, permit of their returning to their natural tone and dimensions. I am convinced that the chief element in this method of cure must be the relief afforded to the over-distended vessels by the removal of *superincumbent* pressure. That the superincumbent weight of the blood does act injuriously, and that its removal must act beneficially on a varix,

¹ On Diseases of the Testis, p. 469.

may be easily demonstrated. To illustrate the point, we prefer quoting a great authority to giving any statement or explanation of our own. The illustration was made on a patient who had an unusually large cluster of varicose veins on the inside of the leg, "while the *vena saphena major* was of enormous diameter. If I put on a bandage," says Sir Benjamin Brodie, "and squeezed the blood out of the veins below, and then put my thumb on the *vena saphena* above, so as to stop the circulation through it, I found, on taking off the bandage, the patient being in the erect posture, that the cluster of veins below filled very slowly, and only from the capillary vessels." But on removal of the pressure, the valves of the vein being useless, "the blood rushed downwards by its own weight, contrary to the course of the circulation, and filled the varicose cluster below almost instantaneously."¹

How long it may be necessary to keep up pressure by a truss, to allow the veins to resume their wonted healthy condition, the case I have given does not show; for the truss, now in use ten months, has not yet been discontinued. From seven to nineteen months would appear to be the period necessary, according to Mr Curling's experience. Much will depend upon the age of the patient, as the same authority has pointed out in his work on the *Diseases of the Testis*, in which he states that the plan is particularly applicable in young persons, whose reparative powers are sufficient to restore the veins to their normal state. Cases occurring in old persons, will, for the most part, only be relieved, not cured. When there is a general lax habit of body, general treatment, as a means of increasing the reparative power, ought not to be neglected. Medicines calculated to give tone to the system, such as iron, quinine, salicin, &c., ought to be given in conjunction with nourishing diet and cold bathing. A cold water *lavement*, practised frequently, is also calculated to be of great service, by unloading, without weakening, the coats of the lower bowel, distension of which is well known to aggravate this complaint. Since directing my attention to this subject, it has been a question with me whether the *pressure principle* might not be applied to varicose veins of other parts. The other day I took advantage of a patient visiting me—upon whom I had operated some time ago according to M. Velpeau's process,² and upon whom the effects described by Sir Benjamin Brodie could readily be produced—to recommend a pad of cork, covered with chamois leather, to be applied in the same manner, but not so tightly, as a tourniquet, over at least two inches of the internal *saphena*, as it passes over the back part of the inner condyle of the femur. My object in applying pressure

¹ "Lectures on Pathology and Surgery." By Sir Benjamin Brodie, Bart. 1846. P. 186.

² I inserted four or five needles in this case, behind a large vein leading from an enormous varicose cluster. This cluster had nearly disappeared, but I saw that another of nearly the same size had formed further down the leg.

over two inches of the vein, is to avoid the effect of a common garter, which is generally believed to be injurious. A short period will show the result of this trial. On the evening of the same day that this man called, I received a letter from Mr Curling, mentioning the fact of relief having been obtained in this way of varicose veins in the upper part of the thigh. I was not aware, when I recommended the plan, that it had been ever tried; but it might easily suggest itself as a feasible plan to any one conversant with the results that have been obtained in varicocele. I cannot refrain from inserting here a portion of this letter, emanating, as it does, from a distinguished ornament of medical literature, as well as from one to whom the profession is indebted for first carrying out and giving publicity to this method of curing varicocele. After stating that he continues to entertain the same favourable opinion of the treatment that he had expressed in the *Medico-Chirurgical Transactions*, he goes on to say,— * * * “In no case of painful varicocele in which the patient has remained under observation, have I failed, sooner or later, in giving relief, by persevering in the treatment. As your experience teaches, success depends upon the proper adjustment of the truss, and a due amount of pressure. Some surgeons, not fully appreciating the principle of this treatment, have hesitated to employ it from apprehension of adding to the evil; but it is a curious circumstance, that, amongst the large number of cases of hernia in which a truss has been worn that have come under my notice, I cannot call to mind one complicated with varicocele.”—(A most convincing proof of the efficacy of pressure, this statement, coming from one enjoying large opportunities of examining cases at dispensaries, hospitals, &c.)—“If the pressure operate injuriously on the spermatic veins, that might be expected to be a common complication. I may mention that, in two or three cases of large varicose veins at the upper part of the thigh, great relief has been obtained from the pressure of a truss. I am glad to learn that you intend making this plan of treating varicocele better known in the north, and I shall look forward with interest to a perusal of your observations on the subject in the excellent *Monthly Journal*.—I am, &c.

“T. B. CURLING.”

Perhaps it may be thought by some that too much is made of the subject of varicocele. This may naturally be thought by the practitioner in private practice, who is seldom consulted on account of the disease. The conclusion very generally come to by such is, that this complaint is of unfrequent occurrence, and not worthy of much attention when it does happen. That the first opinion is erroneous we will immediately prove; and that the second is equally so, may be inferred from the circumstance, that varicocele is an important cause of rejection in both the naval and military services, as well as from the fact, that severe and dangerous operations have been long and frequently had recourse to for the removal of this disease; and that even the most distinguished surgeons (amongst whom Sir B.

Brodie) have thought themselves occasionally warranted to perform the operation of castration, to rid their patients of severe and intolerable suffering.

Through the kindness of my esteemed friend, Mr Marshall, deputy inspector-general of army hospitals, I have been put in possession of some valuable statistics on this subject, not yet published. The returns of Edinburgh and Glasgow, as given below, are, I believe, unique, in respect of their pointing out the relative frequency of varicocele on the right side as compared with the left.

Statistics of Recruiting in Great Britain and Ireland during the Year ending 31st March 1844.

Total number of recruits medically inspected	17,540
Ditto rejected	6,028
Found fit	11,514
<i>Some of the causes of Rejection.</i>	
Hernia (inguinal, femoral, umbilical)	229=26.3 per 1000 rejected.
Varicose state of the veins of the spermatic cord	424=70.3 per 1000 rejected, or 24 per 1000 inspected.
Varicose state of the veins of the lower extremities	582=96.5 per 1000 rejected.
<i>Total Rejections of each Class.</i>	

Statistics of Recruiting in Great Britain and Ireland during the Year ending 31st March 1845.

Number inspected	13,370
Ditto rejected	4,146
Approved	9,224
<i>Some of the causes of Rejection.</i>	
Varicocele	264=63.6 per 1000 rejected, or 19.7 per 1000 inspected.
Hernia	227=54.7 per 1000 rejected.
<i>Total Rejections of each Class.</i>	

Return of the Number of Recruits for the Army inspected at Edinburgh, from 25th March 1817 to 31st December 1822, divided into annual periods, wherein the Number deemed fit for the Service are distinguished from those considered to be unfit, with a Specification of the causes of Rejection.

661	Number inspected for	1817
614	"	1818
597	"	1819
927	"	1820
629	"	1821
1041	"	1822

4469 Total inspected.

Rejected for varicocele on the right side	6
Ditto on the left side	132
Ditto on both sides	2

Total rejected 140

NEW SERIES.—NO. XXIX. NOV. 1848.

2 Q

The following were inspected at Glasgow, from 1st January 1817 to 20th June 1823.

613	Number inspected for	1817
593	"	"	1818
805	"	"	1819
1,138	"	"	1820
788	"	"	1821
1,122	"	"	1822
696	"	"	1823
<hr/>										
5,755	Total inspected.									
4,469	Total inspected at Edinburgh.									
<hr/>										
10,224	Total inspected at both places.									
	Rejected for varicocele on the right side	3
	Ditto on the left side	89
	Ditto on both sides	6
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	Total rejected	98
	Total rejected at Edinburgh	140
<hr/>										
	Total rejected at both places	238

Of the 10,224 inspected at both places, 238 were rejected for varicocele = 2.32 per 1000 of inspected.

It will be seen from these statistics how large a proportion of recruits are rejected on account of varicocele. Moreover, it ought to be borne in mind that only those afflicted with this disease to such an extent as to interfere with their usefulness as soldiers would be rejected, and that, therefore, many slight cases of cirsocele may have been admitted. As a reason for submitting slight cases to the treatment I have explained, may be here stated the great tendency such have to increase.

The disproportion between the number of cases on the right and left sides is well seen in the Edinburgh and Glasgow returns. Mr Marshall says, "Cirsocele seldom occurs except on the left side. I do not recollect having ever seen a well-marked case of it on the right side, although I have examined nearly 30,000 recruits."—(Marshall *On the Enlisting, &c., of Soldiers*. 1839. P. 32.) The case I have given is interesting, in that it details a well-marked case on the right side. And the special claims of the above communication to notice, consist in its pointing out a source of fallacy in the ordinary mode of diagnosis between hernia and this affection, and in its giving the description of a more efficient truss than has yet been used for the cure of varicocele.

ARTICLE III.—*On the Occurrence of Carbonate of Lime in the form of Pearls as a Morbid Product in the Cow.* By THOMAS ANDERSON, M.D., F.R.S.E., Lecturer on Chemistry, Edinburgh.

THE number of the *Medical Gazette* for the 15th September contains a notice of the occurrence of pearls in the urinary bladder of

the bullock; and, as this is an extremely rare morbid product, I have thought it would be advisable to place on record another instance in which they have been observed, and which is in so far different that the gall-bladder was the source from which they were obtained.

The concretions which I examined were given me some years ago, and were marked, "Biliary Calculi from the Cow, from Antioquia, South America." Soon after obtaining them, I made an examination, which showed that they consisted entirely of carbonate of lime and animal matter, and possessed in every respect the structure of pearls; but I hesitated to publish it, as the calculi had already gone through several hands; and not having seen any observations of similar substances, I thought that objection might be taken to the evidence upon which they were assumed to be morbid products from the cow. Now, however, that difficulty is removed by the publication of Mr Taylor's notice.

The calculi were very numerous—I obtained, I should think, as many as sixty or seventy, and a number were also retained by the gentleman who gave them to me. They varied in size from about a tenth down to perhaps a fiftieth of an inch in diameter, and their form was perfectly spherical. They possessed a brilliant golden lustre, and had exactly the appearance of gold beads which had not undergone the process of burnishing. When introduced into nitric or hydrochloric acid, they rapidly dissolved with effervescence, and retain, until completely dissolved, their spherical form and golden lustre unimpaired. The solution is found to contain nothing but lime. A red heat indicates the presence of organic matter. I determined the quantity of organic matter in the usual way by ignition, and then converting the lime, which had become caustic, into carbonate, by digestion with carbonate of ammonia. In this way I found that 2·014 grains lost 0·270, which gives the composition:—

Carbonate of lime	86·6
Animal matter	13·4
	<hr/>
	100·0

It will be observed that this is a much smaller amount of animal matter than that observed by Hatchet in the oyster pearl, in which he obtained no less than 34 per cent. It is obvious, however, that the concretions which I examined were true pearls, differing only from those of the oyster in possessing a golden lustre—a peculiarity dependent, not upon the presence of any colouring matter, for their solution in acids was perfectly colourless, but solely on their mechanical structure. The pearly lustre is well known to depend on the existence of a fine lamellar structure, and may be imitated by the superposition of a number of watch-glasses; and the modification in colour is dependent on the degree of thinness of the plates composing the concretion.

The occurrence of these calculi in the bile as well as in the urine is of some interest, as indicating their probable source. It is not probable that they should be formed in the urinary bladder from the true constituents of the urine; but the likelihood is that they must have a common origin both there and in the gall-bladder. Now Schmidt has recently endeavoured to show that oxalate of lime is secreted by the mucous follicles, and has established its existence in the mucous membranes of the gall and urinary bladders, and of the uterus. It is more than probable, therefore, that the pearls are formed in consequence of a morbid change in these follicles, whereby their oxalate is converted into carbonate of lime. If this be the case, it is possible that they may be sometimes found impacted in their follicles; and in any case in which they may be found loose in the bladder, it would be desirable to look for them there also.

I may observe, in conclusion, that true biliary calculi are very rare in the cow. Out of a considerable number of gall-bladders which I have opened, I have seen one instance only in which a thin tubular concretion occurred, lining the bile duct for about three-quarters of an inch, near the mouth of the gall-bladder. It consisted of cholepyrrhin.

ARTICLE IV.—*Note on a Camphor and Chloroform Mixture.* By T. and H. SMITH, Chemists, Edinburgh.

THE great difficulty, or rather the utter impossibility, of administering camphor in a state of solution in doses of sufficient potency, has been long felt by the profession to be a serious evil. The form of pill, which forces itself on the physician as the only mode of giving large doses of this medicine, is objectionable in many cases, and in others altogether inadmissible. The camphor, being merely in a state of mechanical division, on being set free in the stomach, from its extreme lightness quickly separates and floats about, thus producing in many cases much local irritation in that organ, instead of soothing or arousing the general system.

We have, therefore, much pleasure in laying before the readers of the *Monthly Journal* a formula for exhibiting camphor in doses of almost any amount of strength—certainly as large as any case can require—and that in a state of perfect solution: thereby allowing of a nice adaptation of the dose to the circumstances of each case.

The formula is as follows:—Three drachms of solid camphor are dissolved in one fluid drachm of chloroform. This is, perhaps, one of the most remarkable cases of solution the whole range of chemistry presents to us. The solution is most *rapid and complete*, and the bulk of the liquid is now increased from one to fully four fluid drachms. This solution, rubbed up with the *yolk* of one fresh egg, may be formed into an extremely elegant emulsion by the addition of water, without the slightest separation of the camphor

or chloroform; in fact, no separation of any kind takes place. If to the proportions given above as much water be added as to make a four-ounce mixture, each teaspoonful of the mixture when formed will contain about five and a half grains of camphor, and about two minims of chloroform. The capability of the formula being varied, so that either the camphor or chloroform may constitute the predominating ingredient, must be quite obvious. This mixture can be administered in any ordinary vehicle, such as water, without the occurrence of any separation; indeed, the mixture is as readily and completely effected as cream with tea or coffee. We have tried the effect of several medicinal substances on the mixture. With none of them has any separation been caused.

A weak saline solution, composed of common salt, phosphate of soda, and an alkaline carbonate, mixed readily, as well as a solution of muriate of morphia and sulphate of zinc. With the volatile alkali and acid liquids—such as a weak solution of acetic and muriatic acids—the mixture seems to become more intimate and stable. The mixture with ammonia has stood since its preparation—now fully a week—without any separation. With water alone, however, the chloroform solution of camphor separates in a few days, but they readily unite again when slightly agitated. The solution of camphor in chloroform, although insoluble in water alone, appears in this mixture to be in as complete a state of mixture as the butter in milk when newly drawn from the cow.

It now remains with the physician to ascertain the therapeutic value of the formula. We hope that by its means our knowledge of the action of camphor as a remedial agent may become more full and precise than hitherto.

ARTICLE V.—*Observations on Gunshot Wounds, made in Paris during the Summer of 1848.* By EDWARD WATERS, M.D., F.R.C.P., Edinburgh, formerly Senior President of the Royal Medical Society of Edinburgh.—(Continued from p. 264.)

TREATMENT OF FRACTURES OF THE EXTREMITIES IN CONNEXION WITH GUNSHOT WOUNDS.

b.—Treatment and Apparatus.

DR KIDD of Limerick, in a communication to the *Medical Times*,¹ alluding to the treatment of the wounded in the Parisian hospitals, writes—"The surgery in France is simpler and better, I think, than in our own country: this is what first strikes you going round." With whatever justice this remark may apply to the hospitals he refers to, its groundlessness with respect to the practice of the Edinburgh school is undeniable. Ever since the advent of Syme and

¹ *Medical Times*, August 26, 1848.

Liston, the utmost simplicity, compatible with efficiency, has characterized the treatment there pursued with respect to fractures; and the application of simple dressings to wounds in conjunction with them, where such exist, in a manner which involves no disturbance of the limb on their being changed, has been uniformly enforced. In the Parisian hospitals this is far from being in the majority of instances the case. In the wards of Roux, Blandin, Boyer, Laugier, Giralès, and others, the practice which has so long distinguished the French surgeons, continues to be rigidly followed. A piece of greased linen is first applied to the wounds, and over this some loose charpie; then the "*linge troué*," which is also covered by more charpie with the view of absorbing the pus; compresses follow, and, after them, the many-tailed bandage, enveloping the whole limb; two lateral splints are next rolled up in the borders of a sheet passed under the fractured extremity (*le porte attelles*), until they come into apposition with its sides, leaving only sufficient space for the interposition of cushions of corresponding length and breadth, and composed of bags stuffed with oaten chaff; a third splint and cushion are applied on the anterior surface of the limb: this apparatus is firmly bound together by separate bands of tape, applied and fastened at intervals. In some services, a piece of oil-skin is interposed, to obviate the soiling of the bed and the more permanent portion of the bandages by the discharges. In some wards, this mode of dressing has been applied, not only to fractures of the thigh, but also to those of the leg, fore-arm, and arm; in others, the double inclined plane has been used in fractures of the leg; in those of Giralès, *gutta percha* has been adopted in several cases with advantage, particularly for fractures of the superior extremity; this substance being softened in warm water and then adapted to the limb, and apertures being practised, corresponding to the wounds, by which a free exit for the products of suppuration is afforded. In fractures of the thigh, the apparatus first described has been used in the service of all the surgeons mentioned, though the loaded state of the limb, the enormous proportions which the dressings, when accomplished, presented, seemed but ill calculated for patients in crowded wards during the heat of summer. Each time the dressings were changed, the raising of the limb became necessary for their removal and renewal, constituting a daily source of disturbance, generally occupying at least half an hour, and attended with much suffering to the patients. The accumulation of the discharges, confined to the neighbourhood of the wound by the multiplicity of the applications, generated no trifling amount of effluvia, and apparently offered the most favourable condition for the development of purulent infection, the most frequent cause of mortality in the Parisian hospitals. No extension was practised; the length of the external splint corresponded to that of the limb, and it was not a solitary occurrence to find the limb in one direction and the body of the patient in another, when the time for dressing arrived.

The practice of M. Jobert, in these fractures, is, on the other hand, remarkable for extreme simplicity; extension by pelvic and foot-bands, and the adaptation of pillows, comprising the whole of the apparatus, and permitting the application of dressings to the wounds with the greatest rapidity, and without in the slightest degree disturbing the patient; splints are rarely employed by M. Jobert, reliance being almost entirely placed on position.

In the earlier periods of treatment, M. Malgaigne followed the same practice, with the addition of a splint in the neighbourhood of the fracture. In a case where the shaft of the femur was fractured near its neck, and where union was effected through the medium of the provisional callus, with shortening to the extent of nearly an inch, a case admitted into his service from one of the temporary hospitals on the 11th of July, he adapted the long splint of Desault to the limb, in order to keep up constant extension; the fracture had been previously treated by the double inclined plane.

M. Baudens had recourse to his fracture box in fractures of the leg. This box is open superiorly, and at the side where the limb enters; its two lateral and distal sides are fixed to the bottom or base of the box by hinges; these sides are perforated by two ranges of holes at fixed distances. The fractured limb was fixed on cushions laid in the bottom of the box, and extension maintained, and the position of the fragments regulated by means of bandages passing through the holes in the sides, and then secured. In this apparatus, the condition of the limb was always open to inspection, and the patient could change his position without risk of deranging the fractures; it also afforded every facility for changing the dressings and for the removal of the discharges.

In some instances of fracture of the superior extremities, the position of the member and the apposition of the fragments was maintained by the "*gouttières en fil de fer*" of M. Mayor,—incomplete cylinders formed of iron wire, tinned to prevent oxidation, and embracing about two-thirds of the circumference of the limb; the meshes formed by the wire are large, and permit the removal of the charpie or wadding with which they are padded, as occasion may require. They possess every necessary variety of form, and constitute a firm and light apparatus.

With the exception of the first, each of these various methods of treatment presents advantages which may render its adoption advisable in certain cases. The first is, however, that in most general use; although the cumbersome nature of the apparatus, the confinement of the discharges to the neighbourhood of the wound, and the disturbance of the limb at each successive dressing, are objections which suffice, in the minds of those who are unfettered by the prejudices resulting from a too exclusive course of education, to banish its employment in all cases of compound fracture, and especially in cases resulting from gunshot wounds, which, sooner or later, are always attended by profuse suppuration.

That the long splint of Desault, so admirably adapted for fractures of the thigh, should be comparatively neglected in the country where it originated, while a conviction of its utility is extending itself elsewhere, is a circumstance not unworthy of observation. M. Nélaton is one of those to whose practice this observation does not apply; his mode of putting up fractures of the thigh, where he yielded to the hope of preserving the limb, is shown in the following case:—

A printer, aged nineteen, an insurgent, was admitted into the Hôpital St Antoine, the 24th of June, with a comminuted fracture of the femur, occupying the inferior portion of the shaft; the splinters were neither numerous nor large, and their extraction was effected without dilatation of the wound; the ball had passed out. The long splint of Desault, properly padded, was applied on the outside of the fractured extremity, a corresponding splint on the inside, and a padded foot-piece was fixed between them, to which the foot was firmly attached. The wounds were simply dressed, without compresses or the many-tailed bandage. The limb was perfectly straight, and visible throughout its whole length.

Considerable discussion has taken place as to the applications best adapted to keep within proper limits the inflammation following gunshot wounds. M. Blandin, when stating his preference of cold poultices and cold water to ice, observed, that ice should not be applied to a limb affected with that sort of local stupor which all surgeons are acquainted with, and for this reason considers it contraindicated. M. Roux delivered his opinion to the Academy as follows:—"I do not believe that great advantage is to be derived from the employment of irrigation, or from the application of cold water or ice. The inflammation cannot be prevented; and, even were it possible, care should be taken to avoid such a result. Our efforts should be solely directed to moderate it."

The prophylactic properties of cold, as an agent to ward off inflammation, have been highly lauded by many surgeons; the danger of gangrene from the use of ice, when inflammation and tension are present, has also been set forward. Amongst British surgeons, the superiority of cold water dressings, in the early treatment of gunshot wounds, has been long admitted; and, with few exceptions, cold, in one form or another, may in like manner be said to have been the favourite application here. M. Baudens relies exclusively on ice for limiting the local action, and esteems it the most powerful of all agents for combating traumatic lesions. He applied it, in every case of gunshot wound, as soon as the resulting shock had subsided; and, at this period, without consulting the feelings of the patient. In one or two rare instances only, its first application produced a disagreeable sensation; this, however, speedily subsided, and gave place to an ineffable feeling of ease and well-being, which could only be fully appreciated by being witnessed. This was almost the uniform condition of his patients; and even in those afflicted with wounds of the lungs and of the liver, the relief afforded by its local application, however questionable its utility, seemed to be con-

siderable.¹ It was employed in the following manner:—A piece of linen, spread with cerate, was first applied to the wound, over which a somewhat thick layer of loose *charpie* was placed; this was covered by a single fold of linen, the corners of which were knotted together, so as to maintain in position the two, three, four or more lumps of ice which were laid on it. The pieces of ice varied in size from that of a walnut to a small hen's egg; they were not applied directly to the surface; they melted very gradually, and, when exhausted, the patients renewed them themselves; the lint was thus kept continually moist. Applied in this manner, the result was invariably one of great comfort to the patients, and a look of suffering was, in exceptional cases only, visible throughout the wards. The absence of all disagreeable odour was equally striking—a circumstance in great measure attributable to the facility with which the change of the dressings was managed; the wards were equally crowded with those of the other hospitals, and any thing approaching "*pourriture d'hôpital*" was rarely witnessed. The mode of application of the remedy would appear to modify the effects; at least in a case in the service of M. Velpeau, where a bladder, filled with pounded ice, was suspended so as to come in direct contact with the wound, the result was far from being equally satisfactory. It was a wound of the thigh, and the patient seemed to suffer considerably.

So long as the local temperature continued exaggerated, so long did M. Baudens continue the ice; on applying the hand to the part after its use for some time, the surface generally conveyed a sensation of coolness, rapidly replaced, however, by one of deep-seated and pungent heat, where the persistence in its employment continued to be indicated; no sooner was it attended with a feeling of discomfort, than it was exchanged for a moist compress, which was also discontinued after one or two days. The remaining treatment presented no features of interest.

Irrigation with cold water, after the manner of M. A. Bérard, has been partially adopted in nearly all the hospitals. The wounded limb was laid on oil-cloth, so disposed as to convey the unevaporated water into a bucket at the bed-side; the wound was covered, as in the latter instance, with a piece of linen spread with cerate, and over this a considerable quantity of loose *charpie*. The irrigation was kept up by means of a flexible syphon; one extremity of the syphon was placed in a can of water, suspended at the head of the bed, while the other was placed in contact with the *charpie* covering the injured part, a small piece of linen only intervening, which was attached to this extremity; a cork, fixed near this end of the tube,

¹ In the service of M. Jobert more than one case, where the lung was wounded and where formidable inflammation was lighted up, was successfully treated without local applications, by copious blood-lettings according to the formula "*coup sur coup*."

regulated the dropping of the water. Its use was restricted to wounds which, in the inferior extremity, did not extend above the knee, and, in the superior, above the elbow. In every case its employment was marked by distinct amelioration of the suffering. M. Jobert had recourse to cold applications, but, in the place of ice or water, preferred cold linseed meal poultices, enveloped in linen; these were changed from six to eight times in the twenty-four hours. M. Malgaigne pursued the same practice, and also M. Velpeau; but with the difference that he applied them to the surface without the intervention of a fold of linen, believing that they thus adapted themselves better to the parts, and that the somewhat oily nature of the poultice was not unproductive of benefit. M. Laugier also resorted to cold applications previous to the establishment of the supuration; in one or two instances he made use of the continuous irrigation; more frequently he merely placed a thick compress, wrung out of cold water, between the superior layer of charpie and the many-tailed bandage; the ordinary dressings, already described, were then proceeded with as usual, without the interposition of any substance to retain the moisture, and the wound left till the period of performing the next dressing arrived—certainly a very inadequate attempt to fulfil the indication sought to be realized.

Further multiplication of these details would serve no purpose; sufficient have been adduced to show that the weight of authority is in favour of cold applications. The objection of M. Roux has no just foundation: the action necessary for the reparation of the injury is in no degree counteracted by ice or cold irrigations, the effusion of plastic lymph is in no way impeded, the pus formed is of laudable quality, and the granulations present the most healthy characters under the use of either. With regard to the employment of the cold irrigation, the testimony of M. Nélaton may not inappropriately be cited. He states—"In the service of MM. Breschet and Sanson, I have seen these irrigations continued during ten, fifteen, twenty days, a month even, for the most grave wounds of the articulations, for the most complicated fractures. In many cases I have seen them succeed perfectly; and I have never seen any serious accidents arise which could be imputed to them."¹ M. Blandin prefers cold poultices and cold water to ice, but it is to be observed that he has made no use of the last remedy; he dreads the effects of its application during the period of local stupor, but this period is one of very short duration; at all events, reasoning must give place to facts, and the opponents of this practice of M. Baudens have not been able to adduce a single unfavourable occurrence in support of their strictures.

Ice applied in moderate quantity, according to the method of M.

¹ *Pathologie Chirurgicale*, tom. i. p. 189; Paris, 1844. He adds:—"MM. Breschet, A. Bérard, Pinel Grandchamp, have substituted tepid water for cold in these irrigations; the results obtained have been generally satisfactory."

Baudens, and not loading the part, as when a bladder filled with it is used, appears to possess some advantages over cold irrigation; for instance, it is applicable to parts, such as the thigh and humerus, where cold irrigation is considered inadmissible even by its originator, and a painful sensation, lasting occasionally twenty-four hours, which generally attends the commencement of the use of the latter, is much less frequent and less durable when ice is employed. The danger of gangrene from its employment when inflammation is established, is in great measure illusory—it has been successfully used under such circumstances; with it, as with irrigation, the heat, pain, and engorgement diminish rapidly, and finally disappear. All the objections as yet urged against its use vanish on examination; and, when the benefit which has been derived from it is considered, more particularly the relief from suffering which it has conferred, a perseverance in its employment appears not only justifiable, but may be held to offer great advantages. An important phenomenon, resulting from the local application of ice in the hands of M. Baudens, was a marked sedative influence on the circulation: from being very rapid, the pulsations in a short time were frequently observed to descend to seventy and sixty in the minute. In one instance, when the symptomatic fever had run high, after the use of the ice for some days the pulse was only forty-eight, soft and regular, all suffering was allayed, the tongue was moist, and the general surface of a comfortable and agreeable temperature.

In estimating the value of the remedy, it should perhaps be borne in mind, that the last week of June and the month of July was the period when its action was observed, and also that the experience of the surgeon who more particularly extols its effects has been chiefly acquired in Algeria.

The *Erysipelas*, which showed itself in conjunction with these wounds, was combated by various means; by M. Baudens, by the exercise of superficial scarifications and the application of ice; by M. Velpeau, chiefly by smearing the affected parts with mercurial ointment; by M. Nélaton, by washing the parts with a solution of sulphate of iron (about an ounce to a quart of water); by others, by strong lotions of nitrate of silver or by opiate fomentations. With all these diverse remedies, the success was pretty equal; no decided superiority could be said to mark the action of the one over that of the others.

Hospital Gangrene gave rise to the usual varieties of treatment, but without presenting any particular points of interest. Lemon juice, which Dupuytren was in the habit of resorting to, was often employed to modify the character of the parts attacked. In the early stage, lint saturated with lemon juice was the mode of application with some, as MM. Nélaton and Giraldès. M. Roux and others completely covered the surface of the wound with slices of lemon as a dressing. In both ways it appeared to exert generally a beneficial

action. To destroy the sloughs, M. Jobert employed the acid nitrate of mercury.

Purulent Infection.—In two cases which presented the general symptoms of purulent infection, repeated rigors, &c., without any assignable local cause, M. Nélaton administered the tincture of aconite, and the patients recovered; others similarly attacked died under its employment.

The remedy was continued during several days, more than a week, commencing with one gramme the first day, and augmenting the dose gradually to three grammes.¹ M. Nélaton attributed the recoveries to the aconite; none of the theories, however, which prevail respecting the etiology and nature of the malady, suffice to explain its *modus operandi* in these cases. A tincture possessing the activity of that directed by Dr Fleming in his admirable monograph on aconite,² could not be given in the manner stated without incurring serious danger; none of the physiological effects there described followed its administration, and further experience is required to confirm its possession of any influence in combating the tendency to a fatal termination in purulent infection.—(*To be concluded in next Number.*)

Part Second.

REVIEWS.

1. *An Act for Promoting the Public Health.* 31st August 1848.
2. *An Act to renew and amend an Act of the Tenth Year of her present Majesty for the more speedy Removal of certain Nuisances, and the Prevention of Contagious and Epidemic Diseases.* 4th September 1848.
3. *First and Second Reports of the Health of Towns Commission.* London, 1844-5.
4. *First, Second, and Third Reports of the Metropolitan Sanitary Commission, with Evidence.* London, 1848.

THE important movement for improving the health and physical condition of the poor, which has now for several years engaged the sympathies of the British public, and which has just achieved its first great victory in the enactment of the measure for the improvement of the public health, of which we elsewhere insert an abstract,

¹ From twenty to thirty drops of a tincture, according to its specific gravity, are equivalent to one gramme.

² On the Physiological and Medicinal Properties of Aconite, by Alexander Fleming, M.D. 8vo. London, 1846.

is to so great an extent the offspring of medical philanthropy, and is so largely indebted for its ultimate success to the patient and self-denying exertions of members of the medical profession, that we offer no apology to our readers for occupying their attention with a few additional remarks upon this interesting subject. We are, moreover, strongly impressed with the opinion, that this public recognition of the science of preventive medicine is an event fraught with the most auspicious consequences to the practitioners of the medical art, and one which will, at no distant period, lead to that social elevation of the profession and improvement of medical science, which every right-minded man has so long desiderated, but of the attainment of which many had begun to despair.

Before entering upon an examination of the provisions of the Sanitary Acts passed during the last session of Parliament, it may be useful to present a very brief sketch of the sanitary movement as it has proceeded in this country.

On the appearance of any disease of extraordinary malignity, it has, in this as in other countries, been usual for those inhabitants distinguished for their wealth, influence, or knowledge, to meet together and consult for the common safety. And the temporary committees thus spontaneously formed have, with a few trifling exceptions, hitherto constituted the sole guardians of the public health.

With such an utter absence of sanitary supervision, it will not appear extraordinary that the metropolis and provincial towns, to which the agricultural poor have for many years been attracted in large numbers by the high wages offered in manufactories, should exhibit the most fearful scenes of misery, profligacy, and disease. For the details of these horrors we must refer to the evidence appended to the reports of the Health of Towns Commission, and to other similar sources of information; suffice it here to state, that from the neglect of the authorities, and the short-sighted selfishness of the proprietors of tenemented houses, filth of the most disgusting and noxious nature has been allowed to accumulate in enormous masses, in and near the dwellings of the poor; that no provisions have been made for the removal of refuse, or for the supply of water; that from the excessive crowding of adults of both sexes in small unwholesome lodging rooms, the preservation of decency has been rendered as impracticable as indulgence in habits of cleanliness; and that shamelessness and demoralization have advanced with giant strides among the unfortunate beings thus exposed from their very childhood to the most deadening and polluting influences. Whilst typhus has in these regions of wretchedness become an indigenous and constant scourge, all other diseases of the zymotic class have found there a hot-bed for their propagation; and, from the operation of these various sources of moral and physical disorder upon the industrious poor inhabiting our large towns, the duration of life has been gradually shortened, until at Liverpool the

average age at death of the labouring class did not exceed fifteen years, while the gentry and professional persons attained on an average thirty-five years.

Now the connexion between excessive mortality and the neglect of certain natural essentials to health, such as pure air, water, &c., is no new discovery. To say nothing of the ample treatises on these subjects in the old medical classics, we find in the works of our own countrymen frequent reference made to the importance of diminishing the mortality among the poor by greater attention to their physical comforts. And some time after the benevolent exertions of Howard had been crowned with success, and Cook and Lind had demonstrated the practicability of preventing scurvy, and other diseases of seamen, by ventilation and a judicious choice of food, a society for improving the condition of the poor was formed in London, under the auspices of Wilberforce, Davy, and other persons equally eminent for philanthropy and science. These isolated efforts were not, however, productive of much permanent benefit, and it was not until the advent of cholera in 1831, that any general steps were taken for removing the Augean accumulations from the courts and bye-streets of our large towns. The instinct of self-preservation operating in the minds of the wealthier classes, based upon the fear that the terrible epidemic, once attracted by the fetid squalor of crowded lanes, might subsequently extend its visitation to more spacious and refined residences, produced at that time an unwonted zeal for purifying the habitations of the poor. But this zeal was from its very nature destined to expire with the occasion which induced its manifestation; and typhus, and other diseases which had for the moment been threatened with extinction, rapidly resumed their sway over their favourite localities. With the disappearance of the cholera, boards of health vanished, the regular inspection of the houses of the poor was discontinued; the government ceased to interest itself in the health of the community, local authorities followed the example of the central administration; poor-rates, often the consequence of unnecessary widowhood and orphanage, annually increased, and the tax upon the benevolent public for the support of medical and other charities, already excessive, promised soon to be wholly inadequate to the increasing demand upon it for the maintenance and relief of the diseased and destitute.

For the interests of the poor, and, as time will ultimately show, for the interests of the wealthy and rate-paying classes themselves, it is fortunate that the revelations made during the existence of the last cholera panic were not wholly unproductive. The office of chief secretary to the English Poor-law Commission happened to be filled by a gentleman, who, to great natural ability, and an extensive official acquaintance with the actual condition of the poor, added a considerable knowledge of the principles of medical science, and an earnest conviction of the importance of

applying those principles to the prevention of disease, on a grander scale than had hitherto been attempted. Having obtained the requisite authority from government, and being ably seconded by some distinguished members of the medical profession, among whom may be mentioned Drs Southwood Smith, Arnott, and Kay, Mr Chadwick issued a series of questions to medical practitioners and other competent persons, resident in various parts of Great Britain, and the answers to these enquiries were subsequently embodied in the first "Report of an Inquiry into the Sanitary Condition of the Labouring Population of Great Britain," published by the Poor-law Commissioners in 1842. Almost simultaneously with the preparation of this report, Mr Chadwick instituted "a special inquiry into the practice of burials in towns," which in the following year appeared as a supplementary report, and which, both as regards the demonstration of the evils suspected to exist, and the suggestion of practicable measures for their prevention, completely exhausts the subject. These reports, and the startling facts which they revealed, led to the issuing of a commission, composed of various noblemen and gentlemen, selected without reference to party, who in 1844-5 presented reports comprising a mass of evidence on the actual state of the dwellings of the poor in the large towns, and embodying the results of the commissioners' personal enquiries and observations in a series of recommendations for the improvement of the law in various particulars affecting the health of populous districts. And it is upon these recommendations of the Health of Towns Commission that all the subsequent efforts of various parliamentary leaders to obtain the enactment of a general sanitary measure have been founded. The bill which has recently received the sanction of the legislature, and which is but a very imperfect realization of the wishes of sanitary reformers, is the *fifth* project that has been submitted in recognition of the necessity for accomplishing this long neglected act of national duty. The Marquis of Normanby twice induced the House of Lords to assent to the provisions of a bill on this subject, but it was each time rejected by the Commons. The Earl of Lincoln endeavoured to meet the difficulty by originating the measure in the Lower House; but, in consequence of Sir Robert Peel's resignation of office, was prevented from conducting his bill to a successful issue. And Lord Morpeth, to whom, under the Whig administration, this branch of legislation was committed, after carrying a bill through its second reading in the session of 1846-7, was prevented by the dissolution of Parliament from seeing it passed into law.

Having thus roughly sketched the chief incidents in the progress of sanitary reform, we shall now endeavour to indicate those features of the new sanitary laws which appear to us most worthy of approbation; while we shall, at the same time, not shrink from pointing out the omissions and defects which will, it is to be feared, render the more important of these acts in many places almost wholly

inoperative. And, in the execution of this task, we shall adopt as a standard the recommendations contained in the Second Report of the Health of Towns Commission. The first and most important provision of the Public Health Act 1848, is the creation of a General Board of Health, at the head of which is placed a cabinet minister, for the superintendence of all matters affecting the sanitary condition of the people. This appointment is not only an immediate advantage to the community at a season like the present, when a fearful epidemic is daily expected, but it also constitutes, in the heart of the kingdom, a nucleus from which sanitary regulations, and the organization of a medical police, may ultimately extend throughout the whole country. And in so far carrying out one of the chief recommendations of the Health of Towns Commission, the government is entitled to the warm commendation of the friends of the sanitary cause. But here the defects of the measure become at once obvious. In the first place, the Act contains no provision for the appointment of a medical member of the General Board; and although this slight upon the profession, and fundamental error in the construction of the central administrative body, has since been partially and indirectly remedied by a clause subsequently inserted, *sub silentio*, in the Nuisances Removal Act 1848, under which Dr Southwood Smith has been temporarily appointed as medical member of the General Board, the omission is not the less reprehensible. Then, with regard to the most essential duty of a central board, viz., the superintendence of the local administrative bodies in the execution of the powers conferred upon the latter by this Act, the measure before us is singularly barren; for the only clause which imparts to the General Board of Health a virtual check upon any reckless or ill-judged expenditure of the local bodies is the 119th, which forbids "the Local Board of Health to borrow or take up at interest any sum or sums of money upon the credit of any rates authorized to be made or collected under this Act, without the previous consent of the General Board of Health." Now, we are no supporters of the principle of centralization; we are, on the contrary, among the number of those who attach great importance to the preservation of municipal and other local privileges, and to the administration of the laws, as far as practicable, by persons who have by long residence become intimately acquainted with the wants of each district, and the wishes, the peculiarities, and it may be the prejudices, of its inhabitants. But if a mass of social evils, admitted by all parties to exist, be by different administrations deemed worthy of the attention of Parliament; if the Queen, in successive speeches from the throne, recommend measures for the removal of those evils to the serious attention of the Legislature; and the latter, after examining witnesses by a select committee of its members, and receiving the reports and recommendations of eminent men specially deputed to enquire into this subject, manifest their

wish to carry into effect the suggestions of her Majesty, supporting her ministers by large majorities in every division that has recently taken place on the question,—then it certainly does not appear too much to require, that the persons entrusted with the local administration of the act should faithfully assist in carrying out the provisions of a law deemed essential to the general interests of the state; and that, in the event of their neglecting the duty which they have undertaken, some arrangement be made for its performance by more efficient parties. For these reasons we shall ever consider our sanitary legislation imperfect, until the Central Board of Health be invested with a discretionary power to compel the local bodies to exercise, for the benefit of the public, the authority which the law has deputed to them for that purpose. Under the present Public Health Act, it is evident that the Local Boards, when constituted, may do almost as much or as little as they please. They are compelled to meet once in each month; they are to “cause the sewers, vested in them by this Act, to be constructed, covered, and kept so as to be not a nuisance or injurious to health, and to be properly cleared, cleansed, and emptied;” lodging-houses and slaughter-houses are to be registered; no new houses are to be built without drains and other conveniences; no new streets shall be laid out without the levels having been previously fixed by the Local Board. Offensive trades, newly established, shall be subject to the regulations of the Local Board; offensive drains, &c., shall be cleansed or covered.

These constitute the chief compulsory clauses of the Public Health Act; the permissive powers are much more extensive; but before noticing them, we may perhaps be allowed to say a word on *permissive* legislation generally. The respect and obedience with which the inhabitants of this country are wont to regard the laws have a twofold origin; they arise in part from a belief that no laws are made but such as are required for the public good, and in part also, and in no slight degree, from a conviction of the impossibility of individuals resisting, evading, or treating with indifference the enactments of the legislature. Of late years, however, Acts of Parliament have lost much of their ancient *prestige*; instead of clear, concise, positive *laws*, the parliamentary printers have annually issued a series of bulky documents, with copious instructions for the use of some hypothetical public body, which may hereafter be called into existence, and the members of which “may, if they think fit,” adopt the suggestions of the legislative Mentor. And perhaps no cause has received more injury from this sham legislation, or been more effectually retarded by it, than that of sanitary reform. For proof of this, it might suffice to refer to the various Town Improvement Acts, most of which contain numerous sanitary provisions capable of being enforced so as to prevent many of the evils above alluded to. But an act passed in the session of 1845-6, entitled, “The

Towns Improvement Clauses Act," and which we believe is still in force, constitutes even a more striking illustration of the evil to which we refer. In it the most ample provisions are made for the sanitary regulations of any town within which it may be applied. It is in many respects more complete than the Public Health Act of 1848; but as its introduction into any town requires another Act of Parliament, it is scarcely necessary to add, that it has not yet, so far as we know, been in a single instance rendered available for the public use. Our experience of the inefficiency of "permissive" legislation, therefore, renders us very doubtful whether the majority of the powers vested in the Local Boards of Health will, in the present state of the law, be exercised by them. And as the limited application of the Public Health Act will compel us to enumerate many of those provisions among the other *desiderata* of a satisfactory sanitary measure, we beg to refer to the summary of the Act at p. 338, for a full account of the benefits which the Local Boards may confer upon the inhabitants of the towns which may be brought within the operation of this law.

Notwithstanding the defects which we have attempted to point out in the preceding remarks, we consider this measure as a most important instalment of the great boon of sanitary reform. As such it would command our deepest gratitude, were it not that we in Scotland, as well as our neighbours in Ireland, to say nothing of the English metropolis itself, are excluded from its provisions. We have expressed, in a previous number of this Journal, our regret that so limited a measure should have been introduced, more especially as we can neither suppose that any special sanitary condition of this portion of the country, nor any special obstruction on the part of its local authorities, presented adequate reasons for withholding a measure of so much importance. So great a defect would be felt as a peculiar hardship at present, when a disastrous epidemic has begun to select its victims from among us, were it not that, in the supplementary act, to which we shall presently refer, we have a remedy against this temporary evil. We sincerely hope that by this act, together with the improved police regulations of our two principal cities, we may be, while the scourge lasts, placed in a more favourable position than we had expected up to a recent period; and we have every confidence that our local authorities will show by their acts, that apathy on their part will not stand in the way of any future legislative provision for the sanitary condition of Scotland.

We have endeavoured to indicate the points in which the Sanitary Act of 1848 is defective in principle, and likely to be inoperative in practice. This duty we feel called upon to discharge in consideration of the interests of the public, but certainly in no spirit of unfriendly criticism, being sensible that in this country no legislative measure can be carried, amidst such variety of interests as exists in the case of this reform, without a weight of concur-

rent popular opinion, which, we regret to say, has not been manifested in the present instance. It is to the public opinion, and especially to the enlightened *medical* opinion, of this country that we appeal to place this question in a proper point of view before the Legislature, and, by the suppression of local interests and prejudices which are at variance with the public weal, to pave the way for a more complete measure of sanitary reform. In Scotland, in particular, we have no time to lose; the temporary act should be put in operation to its utmost limits; and every exertion should be employed, when the urgent necessity for its application is over, to induce the Legislature to replace it by a permanent measure of reform of equal or greater efficiency.

The Nuisances Removal and Diseases Prevention Act of 1848, which received the royal assent a few days after the Public Health Act, though allowed to pass without criticism as a mere renewal of a former act which would otherwise have expired, is a much more efficient measure than that which we have just been considering. As before observed, it corrects one important omission in the Public Health Act, by authorizing the appointment of a medical member of the General Board of Health during the continuance of the Order in Council authorizing the operation of this act. As the latter applies to the whole of Great Britain and to Ireland, and the order of the Privy Council putting in force its provisions has already been issued, it may be useful to present a brief outline of its more important provisions. The order of the Privy Council, applying the act to the whole or any part of the united kingdom, continues in force for six months only, but may be renewed. From time to time after the issuing of such order, and whilst the same shall continue in force, the General Board of Health in Great Britain, and the Commissioners of Health in Ireland, may issue such directions and regulations as they may think fit for the prevention or mitigation of epidemic, endemic, or contagious diseases; and may by such directions and regulations provide for the frequent and effectual cleansing of streets and public ways and places by the surveyors, district or assistant surveyors of highways, trustees, county surveyors, and others by law entrusted with the care and management thereof, or by the owners and occupiers of houses and tenements adjoining thereto; and for the cleansing, purifying, ventilating, and disinfecting of houses, dwellings, churches, buildings, and places of assembly, by the owners or occupiers and persons having the care and ordering thereof, for the removal of nuisances, for the speedy interment of the dead, and generally for preventing or mitigating such epidemic, endemic, or contagious diseases, in such manner as to the said Board or Commissioners may seem expedient; and the said Board or Commissioners may authorize and require the guardians of the poor in England and Ireland, and the parochial boards for the management of the poor in Scotland, by themselves, or their officers, or any per-

sons employed by them in the administration of the laws for the relief of the poor, or by officers specially appointed in their behalf, to superintend and see to the execution of any such directions and regulations, and, where necessary, to provide for the dispensing of medicines and medical relief of persons afflicted by or threatened with such diseases. The Poor-law Commissioners in England and Ireland, and the Board of Supervision established under the act for the amendment and better administration of the laws relating to the relief of the poor in Scotland, may require the officers and persons acting under them to execute the directions of the General Board of Health (and Commissioners of Health in Ireland), and shall have the same powers for enforcing the execution of such directions as they now or may hereafter have in relation to any matter concerning the administration of the law for the relief of the poor. *The guardians and parochial boards may appoint or employ, for the superintendence and execution of such directions, officers or persons in aid of the officers or persons employed in the administration of the laws for the relief of the poor; and such guardians and parochial boards respectively, shall defray the expenses incurred by them respectively in the superintendence and execution of such directions and regulations, out of the funds of their respective unions, parishes, or combinations. Persons obstructing the execution of this act, or wilfully violating the directions or regulations of the General Board of Health, are liable to penalties. The act is not to apply to districts or places where the Public Health Act is in force.*

The above are the chief general provisions of this act, and a very brief comparison of them with those of the Public Health Act will probably induce the reader to agree with us, in pronouncing the smaller the better act of the two. This short and simple Nuisances Removal Act, in fact, invests the Board of Guardians and Parochial Boards with most of the powers conferred on the Local Boards of Health by the Public Health Act, while it possesses the very great advantage over the latter of compelling the local administrative bodies to carry its chief provisions into effect. How far the town-councils, whose power as conservators of the public health is virtually superseded by this conversion of the Boards of Guardians into Local Boards of Health, will deem it necessary to exert themselves for the recovery of the public confidence, remains to be seen. We are not without some hope that the steps now being taken by the General Board of Health, through the machinery of the poor-law, may stimulate the municipal bodies to avail themselves without further loss of time of the powers held out for their acceptance in the Public Health Act. In addition to its more general enactments for the prevention of epidemic and other diseases, the Nuisances Removal Act enables any two inhabitant householders to cause the removal of any accumulation of filth, or other nuisance, by addressing a notice in writing, according to a

form prescribed, to the town-council, or trustees, or commissioners for drainage, &c., of any city, town, burgh, or place, or to any other body of a like nature, or to any commissioners of sewers or guardians of the poor, or in Ireland to the officers of health of any parish. And these public bodies must either compel the owner or occupier to remove the nuisance, or do it themselves at the public expense. In Scotland, the notice must be addressed and delivered to the procurator-fiscal of any county, or the procurator-fiscal or the dean of guild of any royal burgh, or the procurator-fiscal of the justices of the peace of any county, or the commissioners of police, or the trustees for paving, lighting, or cleansing any city, town, burgh, parish or place, or the inspector of the poor of any parish.

Having thus cursorily reviewed the acts for the improvement of the public health, passed during the last session of Parliament, we shall now indicate in very few words the chief points to which the attention of sanitary reformers should in our opinion be directed. They are—

1. The establishment of a local board of health in every town containing more than a certain number of inhabitants (say 10,000.)

2. The sanitary supervision of the small towns and rural districts by some existing machinery, such as that constituted by the boards of guardians and parochial boards, aided by medical and other superintending inspectors.

3. The extension of the Public Health Act in a more perfect form to the whole of Scotland and Ireland, and to the metropolitan districts.

4. The substitution of compulsory for permissive powers, in cases where the latter shall prove to be ineffective.

5. The abolition of the tax on light, and the diminution of the smoke nuisance.

6. The investment of the Central Board of Health with a general power of superintending the administration of the act by the local boards, and of compelling the latter to discharge actively and faithfully the duties which they have undertaken.

7. The appointment to every sanitary district of a medical officer of health, and the establishment throughout the country of a system of medical police.

This latter subject is one of such great importance, that we shall, in conclusion, submit to our readers, and, through them, to the public, some of the reasons which render those appointments one of the most urgent requirements of the age. And though we have said nothing on this point when commenting on the deficiencies of the Public Health Act, it cannot in justice to the profession be wholly passed over in silence.

The commissioners for inquiring into the health of towns state (Second Report, vol. i. p. 122), that—

"The most eminent medical witnesses concur in declaring, that it is by the careful observation of the causes of disease and mortality operating upon large classes of the community, that the mode and extent of their operation may be ascertained, and the power of diminishing and preventing them be acquired; that for this purpose the appointment of an officer, whose duty it would be to direct his undivided attention to such causes would, in our opinion, be a public benefit, more especially to the poorer classes, and might be advantageously employed in making investigations into matters affecting the sanitary condition of the district under his charge;" and recommend "that the local administrative body have power to appoint, subject to the approval of the Crown, a medical officer, properly qualified to inspect and report periodically upon the sanitary condition of the town or district, to ascertain the true causes of disease and death, more especially of epidemics increasing the rates of mortality, and the circumstances which originate and maintain such diseases, and injuriously affect the public health of such town or populous district."

In accordance with this recommendation, the bill introduced by Lord Lincoln not only proposed the appointment of a medical officer of health for the discharge of the above-mentioned duties, but also placed the services of this officer at the disposal of the coroner of the district in all judicial inquiries into the cause of death. In the Towns' Improvement Clauses Act, to which we have previously referred, a similar clause is inserted; and in the first Public Health Bill, introduced by Lord Morpeth, the same recommendation was acted upon. But the Public Health Bill of 1848, as first introduced into parliament, did not contain the slightest reference to this subject, or, in fact, to the existence of a medical profession. The duties of a medical officer of health, as laid down by the writer of a report of the Committee of the London Health of Towns' Association on Lord Lincoln's bill (generally understood to be Dr Southwood Smith), are sufficiently numerous and important.

"His primary or fundamental duties are the verification of the fact as well as of the cause of death, the correct registration of both, and the personal examination on the spot of the sanitary circumstances under which death takes place."—"There are," says the writer, "purposes to be answered by making it compulsory on the officer of health to verify in every case, by inquiries made on the spot, the fact of death, of far greater importance than are apparent at first view. In the performance of this duty, the medical officer, chosen, let us suppose, from the highest rank of his profession, and selected on account of his practical knowledge and his eminence in science, must necessarily visit whenever a death takes place the hovels of the lowest persons in the realm. Such a visit would be attended with inestimable benefit both to the individual families in particular, and to the community in general. The effect of such an appointment would be to place the most ignorant, forsaken, and helpless being in the community, in the moment of his utmost bewilderment and desolation, under the direction of an instructed mind; it would be to secure universally and invariably the presence of knowledge and science in the abode into which death follows the footsteps of disease, and in which are present the conditions that are necessary to give instant birth to pestilence, and to arm it with appalling power. With the visits of the policeman, the rent-collector, and the tax-gatherer, the poor are familiar; they see also the union surgeon, and the officer for administering parochial relief; occasionally the agent of some charitable society, and sometimes the clergyman; but there is no responsible officer, whose proper duty it is to visit

them in the season of distress and sorrow, for the express purpose of affording them protection, counsel, and help. The medical officer of health, with the qualifications here supposed, would be such an officer; his mission would be one purely of benevolence, and most healing and beneficent would be the influence of the appointment by the state of such a public servant for such a purpose. It may be necessary to arm him with certain summary powers—with the power, for example, to place the dead body, during the interval between death and interment, under circumstances which will secure it from injuring the living, to enforce ventilation when it is obviously necessary to the safety of survivors, and summarily to remove nuisances which originate and maintain disease.”—“The station and education of the officer of health would be a sufficient security that his professional visit could not afford annoyance to the higher classes, while his visit must often be serviceable to them in various ways.” And, “on the other hand, the experience of the local registrars affords direct evidence that the visit of the officer of health would not be unacceptable to the humbler classes.”

The officer of health would thus in every case ascertain the true cause of death, and the very large number of cases in which the cause of death is now uncertified by qualified medical practitioners, would then be made to form part of the elements of statistical information; while men thus trained to a close observation of the causes of disease, and accustomed to pathological generalization, would, it is to be presumed, speedily enlarge, and impart additional certainty to the science of preventive medicine.

“Further, the correct registration of the fact and cause of death would put an end to false registration, at present not unfrequently made for fraudulent purposes; it would present more trustworthy materials for constructing and correcting tables of assurance; it would facilitate the recovery of the sums of money assured; and, as the present local registrars might act as auxiliaries to the officers of health, it would most materially improve the machinery for local registration in general.”—“There is another service of great public importance which the medical officer of health might perform, namely, a supervision of the established medical parochial relief.”—“Finally, there is one more service which the officer of health might render that deserves particular notice. In the very performance of his duties as a responsible officer, employed to verify the fact and the cause of death, he would necessarily exercise a powerful additional check upon crime.”

The late enormous increase of the crime of poisoning adds considerably to the weight of this last argument; and when it is remarked that, in addition to these uses, the medical officer of health will act as confidential adviser to the local boards, and as a connecting link between the latter and the general board, it must, we think, be obvious that no sanitary measure can work efficiently without the appointment of such officers. Liverpool and the city of London have already spontaneously demanded, and immediately exercised, the power of making such an appointment; and we are satisfied that the most active opponents of the Public Health Act would not have objected to an amendment rendering the appointment compulsory upon the local boards of health.

We have now completed this necessarily imperfect review of a subject which is as yet but in the process of development. For the attainment of the enactments required to complete this, the first chapter of sanitary legislation, the public must mainly rely

upon the exertions of the local associations already formed in many of our large towns, and upon the establishment of similar philanthropic unions in those districts where they have not hitherto been organized. In this work the members of our profession will in most cases be expected to take the initiative, and sure we are that they will every where prove themselves fully equal to the demand made upon them in the name of humanity and religion; and that in future ages no fairer spectacle will present itself to the Christian philosopher and historian, than that of the disinterested patriotism and untiring benevolence of the present generation of medical practitioners.

G. R.

The Treasury of Natural History; or, a Popular Dictionary of Animated Nature, &c. With Nine Hundred Woodcuts. By SAMUEL MAUNDER.

THE works of Mr Maunder are miracles of laborious compilation, and, although not pretending to any of the higher literary or scientific qualifications, have always been so copious and generally so correct in their information, as to have secured for them justly a large share of public approbation. We can state confidently, from an examination of many of its principal articles, that this work is equal, if not superior, to any of its predecessors.

The volume consists of a treatise on the natural history of the animal kingdom, the English names of the animals being arranged in alphabetical order. The work is concluded by an excellent synopsis of practical taxidermy; including directions for the preparing and also for the procuring of specimens of natural history.

The Diagnosis and Treatment of the Eruptive Diseases of the Scalp.

By J. MOORE NELIGAN, M.D., M.R.S.A., Physician to Jervis Street Hospital, &c. Dublin: 1848. 12mo, pp. 55.

THIS little work, to which we have already been indebted in the pages of our *Retrospect*, has been judiciously reprinted from the *Dublin Quarterly Journal*. It is admirably calculated for the use both of students and practitioners, by the clear and simple style of its descriptions. We are glad to see that the author has forsworn entirely the obsolete pedantry of classification, by which diseases of the scalp were erected into a separate class, and then subdivided without end or meaning. He has undoubtedly taken the true view of favus, in regarding it as a parasitic disease, requiring a peculiar soil for its existence, and connected with a constitutional taint; although, for the treatment, we would rather trust to generous diet and warm clothing, along with the proper local manage-

ment, than to the iodide of arsenic, recommended by Dr Neligan ; and we suspect that, without attention to these hygienic influences, little save temporary good will be gained by the exhibition of remedies. We do not understand why the author has placed pityriasis among the inflammatory affections : in a work so decidedly practical in its character, this is surely an erroneous classification. We mention these points as the principal ones in regard to which we differ from Dr Neligan. To most of his opinions we can give a hearty assent, and have no hesitation in recommending this work as an excellent and practical guide, free from all unnecessary and cumbrous book-learning, to the diagnosis and treatment of diseases of the scalp.

Part Third.

MEDICAL NEWS. EDINBURGH OBSTETRIC SOCIETY.

SESSION XXVII.

MEETING VII.—*Wednesday, 12th July 1848.*—Dr SIMPSON, President, in the Chair.

REPORT OF THE EDINBURGH ROYAL MATERNITY HOSPITAL, ST JOHN'S STREET.
BY PROFESSOR SIMPSON.

THE Edinburgh Maternity Hospital was opened in St John's Street in May 1844, and continued in this locality till May 1846, when the Charity was removed to its present premises in Milton House, Canongate. The following Report comprehends an analysis of the obstetric practice of the Institution during the two years in which it was located in St John Street. The Report was drawn up and communicated to the Medico-Chirurgical Society twelve or eighteen months ago ; and its publication was for a time postponed, under the hope that I might find leisure to render it still more minute and extensive. As, however, I have little prospect of fulfilling that wish, I now publish it, with the permission of the Obstetric Society, in the printed account of their transactions. And, I trust, that the continuation of the Reports of the Institution will, sometimes, be drawn up and communicated to the Society by some of the younger and more active medical officers attached to the hospital.

At the time the hospital was opened, the directors appointed to it the following medical staff :—Drs Campbell and Beilby, consulting physicians ; Dr Pagan, consulting surgeon ; Drs Moir and Simpson, ordinary physicians ; Dr Ziegler, ordinary surgeon ; and Drs Bell, Thomson, Niven, and Carmichael, assistant medical officers. One or two advanced pupils were appointed to live in the house as resident house-surgeons.

I. *The Number of Cases.*—Of women delivered in hospital, there were 374. Of women delivered at their own homes in various parts of the town, there were 1101 ; making a total of 1475 women delivered under the superintendence of the Institution. Among these are included 58 cases of miscarriage or premature labour.

II. *The Ages of the Women.*—In 1457 of the cases, the ages of the women were noted. In 18 cases the ascertaining of this point was omitted. The following table exhibits, in detail, the number of patients delivered, with their respective ages :—

NEW SERIES.—NO. XXIX. NOV. 1848.

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Ages of the Women.	Number of Women.	Ages of the Women.	Number of Women.
16	4	33	47
17	10	34	40
18	30	35	49
19	57	36	46
20	64	37	23
21	75	38	33
22	101	39	18
23	90	40	37
24	84	41	8
25	77	42	11
26	107	43	9
27	69	44	6
28	94	45	2
29	69	46	2
30	107	47	1
31	34	48	1
32	52		
Total	1457

III. *The Quickening*.—The average period of quickening, as calculated from the intern cases where it was noted, was 183 days after the disappearance of the last catamenia.

IV. *The Duration of Pregnancy*.—In the intern cases where the data are afforded, the average duration of pregnancy, calculating from the end of the last catamenia, was 273 days. But on the preceding computation little confidence can be placed, considering the class of patients from which the data are drawn.

V. *The Number of the Pregnancy*.—In 1459 cases, the number of the pregnancy is stated, namely, whether it is the first, second, or third time, &c., the woman has been pregnant; in 16 cases it is omitted. 398 women were delivered of their first children; and thus the frequency of first deliveries was to that of all subsequent deliveries in the proportion of about 1 to 4. The following table exhibits at length the number of cases, with the corresponding number of the pregnancy in each.

Number of the Pregnancy.	Number of Women.	Number of the Pregnancy.	Number of Women.
1	398	10	38
2	294	11	24
3	161	12	11
4	158	13	7
5	116	14	2
6	88	15	1
7	65	16	1
8	57	17	1
9	36	20	1
Total	1459

VI. *The Number of Children.*—The children resulting from the 1417 labours, at or near the full time, were 1436 in number. Of these there were born

In single births	1399 children
... twin births	34 ...
... triplet birth	3 ...

Total	1436
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VII. *The Sex and State of the Children.*—The children are further to be distributed as follows :—

Sex of the Child.	Number of Children.	Number Born Alive.	Number Born Dead.	Proportion of Still-births.
Male . .	739	682	57 or	1 in every 13
Female . .	693	650	43 or	1 in every 16
Sex not stated	4	3	1
Total .	1436	1335	101	1 in every 14

Excluding from the above list of still-born children 37 which were putrid, there remain 64 whose death was recent. Of these 64, there were born

Under cranial presentations	40
... breech presentations	8
... footling presentations	4
... arm presentation	1
... shoulder presentation	1
... presentation or prolapse of the cord	8
... placental presentation	1
Presentation not stated	1
Total	64

In one of the cases of still-birth under cranial presentation, the mother had convulsions during labour; and, in another, the child was a male twin; in a third, the cord was twisted twice around the neck; in a fourth, the child exhibited marks of abdominal disease; and in a fifth, the delivery was completed by perforation. Six of the dead children were born in labours complicated with hemorrhage during or before labour.

Of the 37 children born putrid, 18 were male and 19 female infants; and the presentations were as follows :—

The head presented in	26 cases.
... breech	6 ...
... feet	1 ...
... shoulder	1 ...
Presentation not stated in	3 ...

Total	37 ...
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During pregnancy two of the mothers whose children were born putrid, laboured under syphilitic complaints; and a third was affected with ascites and anasarca. In several of the still-born children which I took the opportunity of examining along with the pupils, effusion of coagulable lymph (the consequence of extensive inflammation) was found covering various parts of the peritoneal membrane.

VIII. *The Length of the Children.*—The extreme length or height of the child at birth is stated in 330 cases of single birth completed in the hospital. 169 of these children were male and 161 female.

Of the male children there measured in length—

Under 15 inches, 1 child	Under 20 inches, 53 children
... 16 ... 1 21 ... 28 ...
... 17 ... 3 children	... 22 ... 21 ...
... 18 ... 14 23 ... 3 ...
... 19 ... 45 ...	
Total	169

Of the female children there measured in length—

Under 15 inches, 1 child	Under 20 inches, 40 children
... 16 ... 1 21 ... 32 ...
... 17 ... 1 22 ... 13 ...
... 18 ... 10 children	... 23 ... 2 ...
... 19 ... 60 24 ... 1 child
Total	161

Average length of male child 19½ inches
 ... female child 18½ ...
 ... both sexes 19½ ...

IX. *The Weight of the Children*.—The following table gives in detail the respective weights of 337 children delivered within the hospital, including 171 males and 166 females. The children born in plural births are excluded from the table.

Of the male children there were:—

Under 4 lbs., 4 children.	Under 8 lbs., 60 children.
... 5 ... 7 9 ... 31 ...
... 6 ... 17 10 ... 3 ...
... 7 ... 45 11 ... 4 ...
Total	171 ...

Of the female children there were:—

Under 4 lbs., 2 children.	Under 8 lbs., 50 children.
... 5 ... 4 9 ... 27 ...
... 6 ... 17 10 ... 7 ...
... 7 ... 59 ...	
Total	166 ...

The lightest child (premature?), a male, 3 lbs. 4 oz. in weight.

The heaviest child, a male, 10 lbs. 8 oz. ditto.

The heaviest female child, 9 lbs. 7 oz. ditto.

The average male child, 7 lbs. 3 drs. ditto.

The average female child, 6 lbs. 10 oz. 11 drs. ditto.

The average of both sexes, 6 lbs. 13 oz. 7 drs. ditto.

X. *The Length of the Umbilical Cord*.—In 327 cases of children delivered within the hospital, the length of the cord was noted. Among these—

The shortest cord measured 12 inches in length.

The longest ditto 43 ditto.

The average ditto 23½ ditto.

In a note attached to the details of an extern case the cord is stated to have been 50 inches long.

XI. *Weight of the Placenta*.—In 325 of the cases delivered within the hospital, the weight of the placenta is noted.

The lightest placenta weighed 7 oz. Premature child?

The heaviest ditto, 2 lbs. 15 oz. The single placenta of twin children.

The average ditto, 1 lb. 4 oz. 14 drs.

XII. *The Duration of Labour*.—The duration of labour from the first com-

mencement of pains was ascertained in 311 cases, all delivered within the hospital. The following table exhibits the average length of the whole labour, and of the first, second, and third stages separately in these 311 cases :—

Average duration of first stage of labour, 11 hours 50 minutes.

Ditto	ditto	second	ditto	1	„	37	„
Ditto	ditto	third	ditto	0	„	38	„
Ditto	ditto	whole labour		12	„	57	„

The following table exhibits in detail the duration of the whole labour, and of each stage of labour respectively, in all the cases. It reads thus :—The whole labour was completed in 1 hour in 4 cases ; in 2 hours in 4 cases ; and so on. The first stage of labour was completed in 1 hour in 3 cases ; in 70 hours in 1 case, &c.

Duration in Hours.	Whole Labour.	First Stage.	Second Stage.
1	4	3	161
2	4	9	65
3	7	11	20
4	16	20	16
5	17	26	10
6	16	17	2
7	28	18	4
8	21	18	1
9	17	16	2
10	20	16	1
11	20	15	4
12	12	16	0
13	23	9	0
14	14	9	1
15	8	5	0
16	6	6	0
17	6	3	0
18	8	5	0
19	10	6	0
20	3	7	0
25	22	14	1
30	12	8	0
35	5	5	0
Above 36	14	9	0
Duration not } stated..... }	61	85	103

The third stage was completed in 5 minutes in . . 40 cases.

Ditto	...	10	88	...
Ditto	...	15	80	...
Ditto	...	30	104	...
Ditto	...	1 hour	16	...

Third stage completed within 1 hour in . . 328 ...

Ditto ... 1½ 7 ...

Ditto ... 2 1 ...

Duration not stated in . . . 38 ...

XIII. *The Modes in which the Children presented.*—In 1421 of the births the presentation of the child is given. The following table exhibits in detail the various presentations, and the number of each respectively in 1421 cases :—

Presentation.	Number of Cases.
Head	1333
A hand descending with the head	31
Both hands descending with the head	1
Face	6
Shoulder	3
Arm	3
Breech	27
Breech and feet	2
One or both feet	15
	<hr/> 1421

The cord presented along with some part of the fœtus in 5 cases.

The cord became prolapsed in 13 cases.

The placenta presented partially along with some part of the infant in 3 cases.

In the preceding table of presentations I have entirely omitted to classify those of the cranium, according to the different positions in which the head may present; because I do not place much reliance upon the perfect accuracy of the reports of positions entered in the hospital records by the younger pupils. In fact, one of their principal objects in hospital and dispensary instruction is to acquire, among other things, a practical knowledge of the somewhat difficult subject of positions. But I have great pleasure and confidence in citing the notes on this subject of Dr Martin Barry, who acted for sixteen months as house-surgeon to the hospital, more especially as the data which he has obtained entirely coincide and agree with the results of my own observations. Dr Barry himself carefully observed and noted the position of the head in 335 cases of cranial presentation, among the patients of the Institution. I shall classify these 335 cases according to the four positions and numerical nomenclature used by many of the German schools:—

I. Occipito-anterior Positions.	
1. Position; or occiput directed to left foramen ovale,	in 256 cases.
2. Position; or occiput directed to right foramen ovale,	... 1 ...
II. Occipito-posterior Positions.	
3. Position; or occiput directed to right sacro-iliac synchondrosis ...	76 ...
4. Position; or occiput directed to left sacro-iliac synchondrosis ...	2 ...
Total	<hr/> 335 ...

In the cases of occipito-anterior position the head was invariably found to descend and emerge, without any change or movement from the same oblique diameter of the pelvis as it was originally placed in, with one exception. In this solitary case the head rotated from the first into the fourth position, in which it was born.

Out of 78 cases of occipito-posterior position carefully watched, in 75 the occiput turned forwards, and emerged first from under the arch of the pubis. In 2 cases of the third position the occiput continued directed backwards, and the child was expelled without the usual rotation into the first position. Thus the exceptions to the rotation did not occur oftener than about once in thirty cases.

Of the two cases which presented in the fourth position, in one the rotation was performed into the first position. In the other, the head continued to maintain the same position till it was born.

XIV. *The Plural Births.*—Among the 1417 women delivered, 17 or 1 in every 83 gave birth to twin children, and in one case triplets were born.

In the *twin cases* the presentations and state of the children were as shown in the following table:—

Presentation.	Number of Cases.	Number of Children Born Alive.	Number of Children Born Dead.
Double cranial presentation .	8	13	3
1st child, head ; 2d child, breech	6	11	1
1st child, feet ; 2d child, head .	1	2	
1 child, arm ; other not stated .	1	2	
1 child, head ; other not stated	1	2	
Total	17 cases or 34 children	30	4

The sexes of the twin children were as follows :—

In 3 cases, both children male	6
... 5 cases, ... female	10
... 9 cases 1 child male and 1 female	18
Total 17	34

The average weight of the twin child was 6 lbs. 1 oz.

In the *triplet* case, two of the children presented the head, the third presented the feet. Two of the children were male, one female. All were born alive and vigorous, but, before leaving the hospital, one of the children was accidentally overlaid by its mother. The male children weighed respectively, 5 lbs. 11 oz. and 5 lbs. 2 oz. ; the female child weighed only 3 lbs. 12 oz. The labour was 8½ hours in duration.

XV. *The Mode of Delivery.*—Deducting the abortions and miscarriages, which amounted in number to 58, there remain 1417 cases of delivery about the full time. The following table shows the modes of delivery in these 1417 cases, and their relative frequency to each other. Among those naturally delivered is included a case of spontaneous cephalic evolution or expulsion :—

Mode of Delivery.	Number of Cases.	Proportion.
Naturally	1404	1404 in 1417
Artificially	13	1 ... 107
<i>Mode of Artificial Delivery.</i>		
Traction of presenting feet	1	1 in 1417
Bringing down a foot (breech presentation)	1	1 ... 1417
Turning	6	1 ... 236
Forceps	3	1 ... 472
Crotchet	1	1 ... 1417
Induction of premature labour	1	1 ... 1417

Turning was adopted in five cases for preternatural presentations, and in one case for the purpose of hastening delivery under an attack of apoplectic convulsions. The breech presentation, in which a foot was brought down, was a case in which the presenting part did not pass the brim, notwithstanding strong and powerful pains of upwards of a day's duration.

XVI. *The Complicated Labours.*—The following table contains a list of the chief complications observed in the 1417 labours, with the respective numbers and proportion of each :—

Hæmorrhage from partial placenta prævia, in	3 cases, or 1	in every 472 cases
— before labour,	12 1	118 . . .
— during labour,	4 1	354 . . .
— between the expulsion of the child and of the placenta,	16 1	87 . . .
— post partum,	24 1	59 . . .
— (indefinitely stated),	8 1	177 . . .
<hr/>		
Total of cases of hæmorrhage, ¹	67 1	21 . . .
Convulsions during labour,	4 1	354 . . .
Prolapsus of the entire uterus to the outlet,	2 1	708 . . .
Rigidity of the os uteri,	11 1	128 . . .
Carcinoma of the os uteri,	1 1	1417 . . .
Prolapsus of the anterior lip of the uterus,	6 1	236 . . .
Expulsion of the entire ovum (membranes unbroken),	1 1	1417 . . .
Prolapsus of the cord,	18 1	79 . . .
Cord once or twice around the child's neck, in 158	1 1	9 . . .
— three times around the child's neck,	4 1	354 . . .
— four times do. do. do.	2 1	708 . . .
— encircling the neck and one hand,	1 1	1417 . . .
— around the neck and arm,	4 1	354 . . .
— around the neck and body,	2 1	708 . . .
— around the shoulder,	2 1	708 . . .
— around the body,	2 1	708 . . .
— around the arm,	1 1	1417 . . .
— around the leg,	2 1	708 . . .
— snapped across before delivery (12 inches long),	1 1	1417 . . .
Amatosis and delirium before delivery,	1 1	1417 . . .
Gangrenous slough from the interior of the rectum,	2 1	708 . . .

XVII. *The Instrumental Deliveries.*—Instruments were had recourse to in very few cases. The following table exhibits the number and proportion of the deliveries in the 1417 labours :—

Instrumental delivery,	in 4 cases, or 1	in every 354 cases
Viz. Delivery by the forceps,	3 1	472 . . .
Delivery by the crotchet,	1 1	1417 . . .

Of the four mothers, one died eight days after delivery from puerperal fever. She had been in labour upwards of two days, and was ultimately delivered of a dead child by the short forceps. Of the children extracted by the forceps, two were still-born. In both cases the head had descended into the pelvis before the instruments were applied. In one of these two cases the hospital record states, that, from the state of the cord, the child appeared to have been dead for some time before delivery. It was born after above two days' labour. In the second case the woman had been upwards of twenty-four hours in labour. Convulsions came on an hour and a half before delivery, and returned several times. The child was of very large size, weighing 10 lbs. 2½ oz.

Delivery by the crotchet was resorted to, in consequence of the head of the child being arrested at the brim of a distorted pelvis.

The proportion of cases in which instrumental delivery was had recourse to in the Edinburgh Hospital, was smaller than in most other Obstetric Institu-

¹ In the Hospital books " hæmorrhage " is frequently noted when there was a more than usual loss of blood, although it may have been in comparatively small quantity.

tions. The following table will perhaps illustrate this point more fully than any more lengthened remarks. The first column of figures shows the absolute number of labour cases reported by the different practitioners named; the second column shows the proportion of instances in each of the returns in which the practitioner resorted to delivery by instruments; and the third and fourth columns respectively show the comparative proportion in which the two modes of instrumental delivery (the forceps and crotchet) were employed by each practitioner. It will be seen how frequently the forceps were employed by some in comparison with craniotomy; how frequently craniotomy was employed by others in comparison with the forceps; and how very different the proportion of cases supposed to require instrumental delivery at all, is in the practices of different medical schools and institutions.

Table showing the Proportion of Instrumental Deliveries, and of Deliveries by the Forceps and by Craniotomy in different Obstetric Institutions.

Name of Reporter.	Total Number of Labours Reported.	Proportion of Instrumental Deliveries.	Proportion of Deliveries by Forceps.	Proportion of Deliveries by Craniotomy.
Siebold—Berlin . . .	2093	1 in 7	1 in 7	1 in 2093
Busch—Berlin . . .	2056	1 ... 11	1 ... 12	1 ... 342
Carus—Dresden . . .	2549	1 ... 13	1 ... 14	1 ... 283
Nægele—Heidelberg . . .	1711	1 ... 31	1 ... 31	1 ... 1711
Bland—Westminster . . .	1897	1 ... 95	1 ... 158	1 ... 237
Beatty—Dublin . . .	1182	1 ... 98	1 ... 131	1 ... 394
Collins—Dublin . . .	16,654	1 ... 115	1 ... 617	1 ... 141
Churchill—Dublin . . .	1640	1 ... 117	1 ... 546	1 ... 149
Lever—London . . .	4666	1 ... 137	1 ... 518	1 ... 186
Boer—Vienna . . .	9589	1 ... 199	1 ... 274	1 ... 737
Lachapelle—Paris . . .	22,243	1 ... 252	1 ... 293	1 ... 1854
Ramsbotham—London . . .	48,682	1 ... 322	1 ... 553	1 ... 773
Simpson—Edinburgh . . .	1417	1 ... 354	1 ... 472	1 ... 1417

XVIII. *The Maternal Deaths.*—Among the 1475 women delivered under the superintendence of the Institution, eleven deaths occurred, or one in 134. Of these, seven occurred among the cases delivered in the hospital, and four among the out cases.

Four of these eleven patients were in their first labours; three in their second; and the remaining severally in their third, sixth, ninth, and twelfth.

The cause of death was,—

Puerperal fever and inflammation in	7 cases
Puerperal convulsions in	2 ...
Chest disease in	1 ...
Fatal sinking after incision of a carcinomatous os uteri in	1 ...

Total, . . . 11 deaths.

The delivery was effected by the natural efforts in nine of the cases; by the forceps in one of the cases dying of puerperal fever; by turning in one of the cases dying of convulsions,—the body, on dissection, presenting a large clot of effused blood in the brain, and a collection of pus in one of the kidneys. In the case where the os uteri was incised for carcinoma, the mother had been in labour for two or three days, and the pulse had become very rapid, &c., before the operation was adopted. The child was subsequently born in the course of four or five pains. But the pulse never rallied after delivery, and she died on the third day.

I have merely further to add, that the convalescence of the hospital patients was often interrupted by febrile and inflammatory attacks. The house, No. 3, St John Street, used as an hospital, was, in addition to a sunk kitchen flat, three storeys in height, with two rooms in the first and second flats, and three in the third; and it was much too small for the accommodation of the patients, and of the resident house-surgeons, matron, &c. The rooms themselves were low-roofed, and very imperfectly ventilated. Various plans were attempted to improve the ventilation, but not with much success. Whenever the hospital became in any degree crowded, fevers (or weeds), with more or less abdominal tenderness, appeared among the patients; and frequently the superintention of such attacks during convalescence became so common, as to constitute the rule rather than the exception to it. Every one acquainted with hospital practice, whether obstetric, surgical, or medical, is well aware of the great liability among the patients to febrile and inflammatory attacks whenever the wards are overcrowded; and in no practice is this more visible than in midwifery. Indeed, I believe there are few or no circumstances which would contribute more to save surgical and obstetric patients from phlebotic and other analogous disorders, than a total change in the present system of hospital practice. I have often stated and taught, that if our present medical, surgical, and obstetric hospitals were changed from being crowded palaces, with a layer of sick in each flat, into villages or cottages, with one, or at most two patients in each room, a great saving of human life would be effected. And if the village were constructed of iron (as is now sometimes done for other purposes), instead of brick or stone, it could be taken down and rebuilt every few years; a matter apparently of much moment in hospital hygiene. Besides, the value of the material would not greatly deteriorate from use; the principal outlay would be in the first cost of it. It could be erected in any vacant space or spaces of ground, within or around a city, that chanced to be unoccupied; and, in cases of epidemics, the accommodation could always be at once and readily increased.

PUBLIC HEALTH ACT.

THE following pages contain an account of the most important sanitary provisions of this bill. For a more extended view of its different clauses, we refer our readers to the bill itself, or to the abstract in the *Journal of Public Health* for September 1848, to which we are in great part indebted for the following condensed account:—

General Board of Health.—The act is of limited application, including only England and Wales, and excluding London and the metropolitan sewerage districts. Its first and most important object is to provide a *General Board of Health*, to consist of the first Commissioner of Woods and Forests as president, and two other persons to be appointed by her Majesty, and removeable at pleasure. One of these is to be a salaried officer. The Board is to appoint a secretary, superintending inspectors, and such other officers as they see fit, subject to the approval of the Treasury. The superintending inspectors to have salaries of not more than three guineas a day, besides travelling expenses.

Petition for Inquiry.—The general board may, on the petition of one-tenth of the poor-rate payers ("not being less than thirty in the whole") of any city, town, borough, or place, having a known or defined boundary, direct a superintending inspector to make public inquiry as to the sewerage, drainage, water supply, burial-grounds, number and sanitary condition of inhabitants, existing local Acts of Parliament for sanitary purposes, natural drainage area, existing municipal or other local boundaries, best boundaries for the purposes of this act, and such other matters as the general board may require to enable it to report, or make a provisional order. The general board may also institute an inquiry, and take the right steps thereon, in all cases where the annual mortality of any city, town, borough, parish, or place, shall be found, on an average

of seven years, as shown by the registrar-general's last returns, to exceed twenty-three to a thousand of the population.

Notice of Inquiry and Report.—Before proceeding with the inquiry, the superintending inspector shall give fourteen days' notice of his intention to make the same, and that he will be prepared to hear all persons desirous of giving evidence. Public notice to be by advertisement, and by fixing notices to the doors of the principal churches and chapels, and also on the public buildings. The inspector shall then report to the general board; and if it shall appear that the best boundaries for the purposes of this act are not the same as those of the city, town, borough, or place reported on, a superintending inspector shall visit and examine the parts "within the boundaries proposed to be adopted," in the same manner and after similar public notice. And after he has reported to the general board, the report shall be published in the parts to which it refers, and copies shall be deposited with the town clerk of any corporate borough, with the clerk to the justices of any petty sessional division, and with the clerk of the board of guardians of any union or parish affected thereby: and with the clerk to any board administering any local act for sanitary purposes.

Application of the Act.—After the inquiry and report above mentioned, her Majesty may, by an order in council, or a provisional order, direct the application of the provisions of the Act to the district reported on; the consent of the local authorities being unnecessary, except where a change of boundaries is proposed.

Local Boards of Health.—In corporate boroughs, these are to consist of the mayor, aldermen, and burgesses; in non-corporate boroughs and extra-municipal districts, the board is to consist of members elected by the ratepayers, to a number fixed by order in council or provisional order. The members must be resident within the district, and must possess certain other qualifications mentioned in the Act. The local boards are to hold meetings at least once a month.

Officer of Health.—The local board may appoint "a legally qualified medical practitioner, or a member of the medical profession," to be their officer of health, removeably by them, and who shall perform such duties as the *General Board of Health* shall direct. He may be officer of health for two or more districts, and such salary or other remuneration shall be paid to him out of the general district rates, as the local board or boards may direct, and where there are more than one district under his charge, in such proportion from each as the general board may appoint. The appointment and removal of the officer of health to be subject to the approval of the general board.

Sewers.—The local Board of Health shall repair and maintain all sewers vested in them, and shall cause to be made such main sewers and drains as are necessary for effectual drainage of their district. After notice in writing, such sewers may be carried through any property. Existing sewers may be discontinued, provided no private right be interfered with, and in such case an equivalent sewer must be provided. The local board shall cleanse all sewers and drains belonging to them, and construct such reservoirs, sluices, engines, &c., as may be needful. It may cause sewers to be emptied into needful places, and the manure sold; but not to create nuisance.

House-drains, Privies, &c.—No house shall be built or rebuilt without sufficient drainage, on report of the surveyor, and if the sea, or any sewer or drain which the local board can make use of, be within 100 feet of any part of the site of the house, the drain from the house must communicate with either, as the local board shall direct. Where there is no main drainage, then a cesspool must be constructed to receive the house-drain, at such a distance from the house as the local board shall direct. Neglect of these provisions shall incur a penalty not exceeding L.50. All existing houses shall be similarly drained, and if the requisite drainage be not completed after due notice, then the local board may execute the works, and recover the expense from the owner in a summary manner, or as private improvement expenses.

No house shall be built or rebuilt without a sufficient water-closet or privy, and an ash-pit furnished with proper doors and coverings, under a penalty not exceeding L.20, for neglect of the law. In existing houses, where there are not sufficient conveniences, the local board shall also give notice to the owner or occupier, and if within reasonable time the proper conveniences be not constructed, the local board may execute the same, and charge the cost.

All places of manufacture, trade, or business, where above twenty persons of both sexes are employed at the same time, must be provided with separate water-closets or privies for the use of each sex. And if, after due notice from the local board, the proper conveniences be not constructed, a penalty not exceeding L.20 may be inflicted, and a further penalty of L.2 a day during the time of default.

The local board shall provide that all drains, privies, &c., within their district do not occasion nuisance.

If any person make a written application to the local board (but not otherwise), complaining of any nuisance arising from any drain, water-closet, privy, cesspool, or ash-pit, the surveyor, by written authority of the local board, and after twenty-four hours' notice, or, in case of emergency, without notice, may enter the premises, with or without assistants, and make examination. If every thing be found in proper order, the damage must be repaired at the expense of the local board; but, if alteration or amendment be required, the ground shall be closed, and the local board shall send notice to the owner or occupier, giving reasonable time for the purpose; and if the notice be not complied with, a penalty of 10s. a day may be levied, and the local board may execute the necessary works, and recover expenses from the owner.

Street Cleansing.—The local board shall at all convenient and proper times cause a thorough surface cleansing and watering of all streets, &c., and it may make bye-laws for the removal of refuse from, or about houses, stables, cow-houses, &c., and for preventing accumulations of such matters, and also for cleansing and emptying of water-closets, cesspools, &c. Bye-laws regulating removal of manure, &c., by occupiers, may also be made.

Places of Deposit.—The local board may provide convenient boxes and places for deposit of refuse matters, sewage, &c., which shall be vested in the board and sold, and the proceeds carried to the district fund account. All persons removing refuse without consent of the local board, shall be liable to a penalty not exceeding L.2 for every offence.

Public Necessaries.—The local board shall provide public water-closets, privies, and other conveniences, in proper situations, and charge the cost to the district rates.

Offensive Drains to be cleansed.—The local board shall cause to be drained, cleansed, covered, or filled up, all ponds, pools, open ditches, drains, and places containing or used for the collection of any drainage, filth, water, matter, or thing of an offensive nature, as likely to be injurious to health. Written notice is so be sent to the person causing nuisance, or to any owner or occupier on whose premises it exists, and in default of compliance the local board shall execute the works, and charge the cost. But the local board may order the whole or a portion of the expenses to be paid out of the special or general district rate.

Penalties for keeping Swine, &c.—The local board may serve notice of nuisance on any person keeping swine in a dwelling-house, or in any other way so as to occasion nuisance, or who permits water to accumulate in any cellar or place within a house, or who permits any soakage or overflow from any water-closet or privy; and if in twenty-four hours after such notice the nuisance be not abated, a penalty not exceeding L.2 may be inflicted, along with 5s. for every day during which the nuisance is continued, and the local board shall abate the nuisance and charge costs on the occupier. The inspector of nuisances shall also give notice of all collections of refuse causing nuisance, and if they are not removed in twenty-four hours, they shall be removed, and be-

come the property of the local board, the proceeds being carried to the district fund account.

Purification of Houses.—Any house, or part of a house, which is certified to the local board by the officer of health (if any), or by any two medical practitioners, to be in a filthy or unwholesome condition, and tending to produce disease, or if disease may be checked by white-washing or purifying the same, it may be ordered by the local board to be so purified, on written notice to the owner or occupier; and in default, after the specified time, the offender shall be liable to a penalty not exceeding 10s. a day, and the local board may cause the necessary purification to be made, and the cost recovered from the owner or occupier.

Slaughter-Houses, &c.—All slaughter-houses and knackers'-yards shall be registered by the owner or occupier in a book kept by the local board, within three months after the application of this Act, or within three months of the time of use, under a penalty not exceeding L.5, and 10s. for every day after notice from the local board.

Local boards may provide public slaughter-houses, and make bye-laws for their regulation, and also for the regulation of all slaughter-houses and knackers'-yards, for the cleansing of them; but the rights of all companies incorporated by local acts to be respected.

Inspector of Meat, &c.—The inspector of nuisances may at all reasonable times enter, with or without assistants, and inspect any place where butcher's meat is slaughtered or sold, or where game, poultry, or fish is sold, and may examine their contents, and seize all meat, poultry, game, flesh, or fish unfit for food, and any justice may cause the same to be destroyed, or kept from being sold as food, if he think fit, after receiving evidence from some competent person; and the individual in whose custody unsound meat, poultry, game, or fish is found, shall be liable to a penalty not exceeding L.10 for every separate piece so found, to be recovered before two justices.

Offensive Trades.—No offensive trade shall be newly established in any building or place, after the application of this Act, without the consent of the local Board of Health, unless the general board shall otherwise direct, under a penalty of L.50, and L.2 a day; and all such works shall be under the regulation of the local board.

This Act does not affect the present law as to nuisances.

Lodging-Houses to be Registered.—Every common lodging-house must be entered on a register kept by the local board, which shall make regulations for fixing the number of lodgers, and for proper cleanliness and ventilation. Officers appointed by the local board shall have access, at all times, to such houses, for inspecting and disinfecting the same, at the expense of the occupier; and if admittance be refused in the daytime, between eleven and four o'clock, the offender shall be liable to a penalty not exceeding L.2. Neglect of registration to be visited by a similar penalty.

Regulation of Cellars.—No cellar, to be built or rebuilt after the passing of this Act, can be let separately as a dwelling, or no cellar which has not been so let before the passing of this Act. And no existing cellar shall be let separately as a dwelling, except it be seven feet high, and have three feet of its height above the level of the street; provided also that there be an area in front two feet six inches in width, and the whole height from six inches below the level of the floor of the cellar to the level of the street; and in any case there must be a sufficient drain laid at least one foot below the floor of the cellar, along with a proper fire-place, water-closet or privy, and ash-pit. The window must be nine feet clear of the frame, and made to open; and if there be a back cellar, the window must be four feet clear of the frame. The churchwardens and overseers to give public notice of the law, and the local board to give written notice to proprietors, and in default of compliance a fine of 20s. a day may be inflicted. Steps for access may be constructed, so as not to obstruct the window. Any cellar or vault in which a person passes the night, is a dwelling

under this Act. The Act not to be in force as to cellars till one year after its passing, nor till six months after its application to any district.

Management of Streets.—All present or future streets and highways, with their materials, are vested in the local board, whose duty it shall be to see that they are properly levelled, paved, flagged, channelled, and repaired, and proper fences kept for the safety of foot passengers. Wilful injury, without consent of the board, to be punished by a fine not exceeding L.5, and 6s. for every square foot injured.

Any present or future street (not being a highway) which is not properly sewered, paved, &c., must be put in a proper state, on notice being served on owners or occupiers of premises fronting or abutting on the defective parts of the streets; and if the notice be not complied with, the local board may execute the works, and charge costs upon owners, according to their frontage, and under advice of the surveyor, or by arbitration.

Any street (not a highway) which may have been paved, &c., to the satisfaction of the local board, may be adopted by the board on public notice, and repaired afterwards out of the rates; but the proprietor of a private street may object within a month after public notice.

The local board may compel any alteration in the position of gas and water pipes, paying the cost out of the general district rate, unless there be legal liabilities on companies; but the use of the pipes is to be preserved unimpaired.

Before laying out any new street, a month's notice must be given to the local board, with the level and width of the street, which shall be fixed by the local board. All persons contravening this clause, are liable to a fine of L.20 a day, and to have alterations made in their property, to bring it into conformity with the Act, and the cost charged upon them. But if the notice be not replied to in one month, parties may proceed to lay out the street; and, should they feel themselves aggrieved by the proceedings of the local board, they may appeal to the general board.

The local board may purchase such premises as may be needful for improving lines of street; but any portion more than necessary must be re-sold, for the benefit of the district-fund account.

Pleasure Grounds.—The local board may purchase and keep in order grounds for public recreation, or pay towards the support of such grounds, provided by any person whomsoever, under approval of the general board.

Water Supply.—It shall be lawful for the local board to provide a constant supply, at pressure, of pure water, for the purposes of this Act. It may erect new works, contract with companies either for supply or lease of works, or it may purchase water-works, &c., with the approval of the general board. But before laying down any new works, the local board must give notice in writing to all water companies within whose limits it requires a supply, stating the amount required, &c.; and so long as the companies can supply enough, and on such terms as the general board shall deem reasonable, or as may be agreed on by arbitration, the local board cannot lay down new works.

If, on report to the surveyor, it appear to the local board that any house is inadequately supplied with water, and that it is possible to obtain a supply on terms not exceeding twopence a-week, notice to supply shall be given to the occupier; and, if this be not complied with, the local board may execute the necessary works, and levy a water-rate not exceeding twopence a-week, and charge the cost of works as improvement expenses.

The local board may supply water for trading or manufacturing purposes, or for public baths or wash-houses.

The local board may maintain and supply with water all cisterna, &c., for gratuitous use, and construct such public works; and may also supply all public baths and wash-houses gratuitously.

Injuring water-works, or unlawfully diverting and appropriating water, shall be punished by fine; but existing rights to streams to be respected.

Any person bathing, washing, or throwing impurities of any kind, into any

water-works belonging to the local board, shall be punished by fine; or any gas or other manufacturer allowing refuse matter to injure water, to be subject to heavy penalties.

Interments and Burial-Grounds.—The local board may provide reception-houses for the dead, and make bye-laws for their regulation; and may also, on application, inter decently and economically any corpse placed in such reception-houses.

If, on representation from the local board, the General Board of Health issue and publish a certificate, to the effect that any burial-ground or vaults under a church are overcrowded with dead, so as to be dangerous to health, no future interment shall be permitted in them, except in specific cases mentioned in such certificate, under a penalty of L.20. The general board, before issuing the certificate, must direct an inquiry, notified to the bishop of the diocese, and publicly advertised, to be made by a superintending inspector or otherwise.

No interment shall take place in or under any place of public worship, built after the passing of this Act; and no burial-ground shall be made in any district after the passing of this Act, or shall be used for interment, unless on land purchased for the purpose before the passing of this Act, without consent of the general board, and under a penalty of L.50 and costs for each offence.

Rates.—The rates for carrying out the provisions of the bill are to be levied in the individual districts, under the authority of the local boards. All property liable to poor-rates is to be assessable. Unoccupied premises are not to be charged. Assessments may be levied on any portion of a district, for specific purposes. Whenever the local board has incurred expenses for the improvement of private property, it may levy a rate to repay principal and interest in thirty years, to be called a *private improvement rate*. It shall be levied on the tenant during his occupancy, and, when premises are vacant, on the owner. In all cases where the occupier is rack-rented, he may deduct three-fourths of the private improvement rate from the landlord, and in other cases in proportion. Rates may be reduced or omitted, in case of poverty. In cases of small tenements (not exceeding the annual value of L.10), or premises let to weekly or monthly tenants, the Local Board of Health may compound with the owners for the payment of rates at not less than two-thirds or more than four-fifths of the net annual value of the property as assessed; the occupier of the premises being also in this case made liable for the payment of the rates to the extent of the rent actually due by him.

General Superintendence.—The superintending inspector, during any inquiry for the purposes of this Act, may examine on oath any person whomsoever, and he may require public officers, poor-law guardians, or commissioners, to produce before him surveys, plans, sections, rate-books, and other documents, and he may take copies. A fine to be inflicted for resistance to his order. All persons attending an order of the inspector to be paid reasonable charges, and no person to be compelled to go further than ten miles from his house.

NUISANCES REMOVAL AND DISEASES PREVENTION ACT.

WE have given in p. 323 of the present Number, an account of the principal provisions of this excellent enactment, which, we beg to remind our readers, has been already put in force in Great Britain and Ireland by a special Order in Council. We subjoin in full the form of notice, by which, under this act, any two householders can cause the authorities to make inquiry as to any nuisance, and, if necessary, ordain its removal.

“To the Town-Council of the Borough of
(or Guardians of the Poor of the
Union, or of the Parish of
County of
or, in Scotland, to the Procurator-Fiscal of the County of

in the
or as the case may be;

or to the Procurator-Fiscal or Dean of Guild of the Burgh of
or to the Procurator-Fiscal of the Justices of the County of
or to the Inspector of the Poor of the Parish of
or as the case may be.)

"We, the undersigned (Inhabitants (or, in
Scotland, Householdiers) of (insert the Parish
or Place), and residing at , in (insert the Parish
or Place before mentioned) aforesaid, do hereby give you Notice, that to the
best of our Knowledge and Belief a Dwelling-House (or Building) situate at
No. in Street, in aforesaid
(or such other Description as may be sufficient to identify the Premises), is in such
a filthy and unwholesome Condition as to be a Nuisance to (or injurious to the
Health of) A. B., or as the case may be; (or that upon certain Premises situate
at (inserting such a Description as may be sufficient to identify the Premises)
there is a foul and offensive Drain, Ditch, Gutter, Privy, Cesspool, or Ashpit,
or a Drain, &c., kept or constructed so as to be a Nuisance to us, or to the Oc-
cupiers of the Premises adjoining the Premises aforesaid, or as the case may be;
or that upon certain Premises situate at, &c., Swine, or an Accumulation of
Dung, Manure, Offal, Filth, Refuse, or Matter, or as the case may be, are or is
kept, so as to be injurious to our Health, or to the Health of A. B., or of the
Occupiers of the Premises adjoining the Premises aforesaid, or of Persons living
in the Neighbourhood, or of the Persons living in the Premises aforesaid, as
the case may be; or that upon certain Premises, &c., Swine, &c., are kept so as to
be a Nuisance to us, &c., as the case may be). And we hereby require that you
will cause such Proceedings to be taken as are directed in this Behalf by the
'Nuisances Removal and Diseases Prevention Act, 1843.'

Dated this day of
hundred and

One thousand eight
"A. B."
"C. D."

TRIAL FOR CHILD-MURDER BY POISONING WITH HEMLOCK—ALLEGED
INNOCUOUSNESS OF HEMLOCK.

HANNAH BOWER, aged twenty-two, was tried at Ipswich, August 3, before Mr Baron Parke, on the charge of murdering her illegitimate child, about two years of age. She had never shown much maternal affection for the child. She had been heard frequently to express herself rancorously towards the child, and even threaten to kill her, as an obstacle to her living comfortably with a new paramour. A few days before the child's death, she predicted that she would not trouble her much longer, and even spoke of her intention to poison her with hemlock. On the day before the girl's death, she got an order for the Union doctor to visit her, though at that time, and up to two o'clock the next day, she was to all appearance well and happy. On the day of her death (May 1), the mother went into the fields to gather hemlock, with the help of a girl, who pointed it out to her, and to whom she said that the child's feet were sore. From her own confession, which, though rejected at first by the judge on account of some informality, was at length put in, it appeared that she was persuaded to kill the child by Glasscock, her paramour; that she showed him the hemlock she had gathered; that he said it was sheep's parsley, and would do as well as hemlock; that by his directions she boiled it, and gave half a cupful of the liquid to the child while it sat in the chair. At two o'clock on the day of its death, the child was seen playing about the cottage door, while the mother stood by, venting her usual feelings in regard to it. The child was then seen to run into the house; the mother followed, and gave her a crust of bread, and immediately she was heard to cry, and in about two minutes from the time of their entrance into the house, the mother came out with the child in her arms, and then it was evident that something serious had befallen her: "her eyes protruded, as if she was scared; her limbs hung

lifeless about her body, and all her muscular powers seemed to have been in one instant paralysed." The mother carried the girl to a doctor, saying that she had bowel complaint, for which he prescribed. She went through the form of giving the medicine prescribed; but the child continued to droop, and died in about an hour from the first occurrence of the symptoms. It does not appear from the report whether any examination of the stomach was made before the body was buried; it is stated, however, that the contents of the stomach were thrown away, so that no medical opinion was obtained in regard to these; the body, however, was exhumed, and the stomach and viscera sent for examination to Mr Taylor of London, and these appear to have been examined also by some medical gentlemen at Harwich. No evidence of the presence of poison was found by any of these parties. Mr Taylor, at the trial, said that the absence of vegetable matter both from the stomach and viscera was not conclusive against the administration of a poison decocted from plants; for it was possible, though not probable, that the fluid might have been carefully strained, or skimmed with a spoon, and that so none of the vegetable particles would pass into the body of the deceased. Mr Taylor further spoke to the symptoms which would follow upon the administration of hemlock as being analogous to those detailed by the witnesses who saw the child carried by the mother to the doctor, and at the time of her death. Mr Martin, however, one of the medical men who had examined the stomach at Harwich, expressed his doubt whether hemlock was of so dangerous a quality as was supposed by many chemists; and stated that he had administered a decoction, made from a quantity of that plant—about equal to that which the prisoner was supposed to have employed—to a rabbit, and that the animal had not suffered the slightest inconvenience from the dose. He also deposed to having been informed by a gentleman last night, that he had been in the daily habit of eating the leaves of the hemlock plant for a considerable time past, and that he had not experienced any bad effect from such treatment.

The jury found the prisoner not guilty, adding, "we entertain a doubt, and give her the benefit of it."—*Times*, August 7, 1848.

We cannot but feel surprised at the verdict in the above case. There does not appear to have been any legal difficulty in the evidence. The presiding judge was plainly satisfied that it was quite competent for the jury to bring in a verdict of murder against the prisoner. What the doubt was, the benefit of which the jury gave to the prisoner, is not stated. The report of the trial is not explicit enough to enable us to discover if the poison, of which the child died, was really hemlock. It seems certain that death took place in consequence of the administration of a poison, supposed by the mother to be hemlock, and that the drug was administered for the purpose of taking away the child's life. In trials for poisoning, the necessity for proving the species of poison employed is not absolute; this proof is often rendered imperative, only because in most cases without such proof the fact of the poisoning cannot be established. Here the proof of wilful poisoning is complete, whether the poison were hemlock, or some of the poisonous plants which resemble hemlock. There is no room for doubt, unless the indictment was so drawn as to exclude from the charge every other poison but hemlock; and, had this been the case, the judge would hardly have left it to the jury, of their own motion, to acquit the prisoner on this score. But most probably the doubt was raised in the minds of the jury by the evidence of Mr Martin, one of the medical witnesses. Mr Martin says he doubts if hemlock be of so dangerous a character as was supposed by many chemists. We know that inaccuracies are apt to creep into the reports of medical testimony in courts of justice, and we should be sorry, on the mere ground of a newspaper report of his evidence, to arraign Mr Martin for ignorance of what should be known by every medical witness. But Mr Martin owes it to himself, and to the profession, to publish what he

said, if the report be inaccurate. We cannot conceive that any medical man could have glanced over the article hemlock, in so accessible a book as Taylor on Poisons, without feeling distrust in his own single small experiment on a rabbit, and in the testimony of a friend, so eccentric as to prefer hemlock with his morning bread and butter to water cresses. The poisonous qualities of hemlock admit of no doubt; and, since the publication of Dr J. H. Bennett's well reported case, no room has remained to question the identity of the common hemlock of our hedges with the Athenian state poison by which Socrates suffered death.

TRIAL OF JAMES M'WHEELAN, OR M'QUEEN, FOR MURDER, AT Ayr.

THE interest of this case lies in the conclusiveness of purely circumstantial evidence.

The body of James Young, a young man of sixteen years of age, the son of a farmer, and farm-servant to a neighbouring farmer, was found on the road near his master's house, about four o'clock in the morning of May 27th, cold and stiff, with a wound in the neck, and two pools of blood close by, one near the head, another about a yard distant. The pockets were empty, and seemed to have been turned out; there was no watch, watch-chain, or guard on the body,—beside it there was a bundle containing a pair of new trousers. On the other side of the hedge, twelve yards from the body, was found a chisel clotted with blood, both on the blade and handle, and beside the body there was a stone, different from the stones around, about the size of a large potato; it was cleaner than the stones around, being free from dust, and had a sweaty appearance, as if it had been carried in the hand. On the same morning, about a mile nearer Kilmarnock than the place where the body was found, or nearly two and a half miles from that town, a handkerchief was picked up from the road-side, with marks on the road beside it as if a person had sat there. On the evening before the deceased had been with his father and elder brother, and had left his father's house some time before eleven o'clock, to go to his master's, a distance of about two and a half miles—and he then had his watch, watch-chain, and guard, and there was strong reason to believe he had some silver in his pockets, the remainder of his wages, which he had received not long before.

About eleven o'clock on the same morning on which the murdered body of the deceased was found, the accused applied to an acquaintance in Beith to pawn a watch for him—which he did, and brought to him the proceeds, namely, one pound. This watch was proved satisfactorily to be the watch of the murdered man—the watch which he had in his possession late in the evening before he was found dead on the road. It was proved by the evidence of a barber, that an hour before, on the same morning, the accused had come to be shaved, and had desired his whiskers, which were very large, and also the hair below his chin, to be removed—the same barber had shaved him several times before, and but a small part of his face around his mouth had then been shaved—the shaving on the morning of the 27th May, made a considerable change in his appearance—the barber remarked to the rest of his customers, as M'Wheelan went away, that he had a strange and startled look. The evidence of the barber was confirmed by that of a surgeon, who had seen him that morning both before and after his whiskers and superfluous hair had been removed. The accused was traced most distinctly from Beith backwards to Kilmarnock, a distance of about fourteen miles, in the course of the morning of 27th May—at ten o'clock he was seen entering Beith by the Kilmarnock road; at nine o'clock he was seen two miles from Beith on the same road—at six o'clock in the morning he was at Dunlop, five and a half miles from Beith—at three o'clock in the morning he was seen by a person who walked for some distance along with him from the toll-bar at the end of the town of Kilmarnock on the Beith road. Between twelve and one o'clock A.M. he overtook a person in Kilmarnock who was walking smartly home in the direction leading from the Ayr road, the

accused coming up behind at a quick pace; he invited this person, with whom he claimed a slight acquaintance, to join him in a glass of whisky—accordingly they adjourned to a spirit shop, and, while so engaged, his companion remarked that there was blood on the sleeve of his left arm, saying, “You have surely been murdering a man, or killing somebody’s swine;” and when the accused turned round he saw a large stain of blood on his left knee, as if it had been dipped in blood—the accused said that he had had a quarrel about a lass, and, drinking off his glass, went quickly out; his companion waiting a little in expectation of his return—but he never came back. The handkerchief discovered on the road from Kilmarnock to Ayr on the morning of the murder, was not certainly identified as belonging to the accused; but several witnesses deposed to his having had in his possession, a few days before, a handkerchief exactly like it—and one who saw him with a handkerchief on the morning of the 26th of May, said, that if he had found that handkerchief after the prisoner left the place where he was, he would certainly have suspected that it belonged to him. It was, moreover, proved that the prisoner was carrying about with him, some days before, a chisel exactly like that which was found covered with blood near the place of the murder. He had at the same time, about his person, a pointed steel rod, termed a widener, which was found on him when apprehended—and when asked, some days before, as to the use he intended to make of these instruments, he used some mysterious expressions, intimating, that if the one did not suffice, the other would. On one occasion, some days before the murder, a miner took both the instruments into his own hand, examining them, and at the trial he identified the chisel found beside the dead body as that which he had taken from the prisoner’s hands for examination on that occasion; he then remarked a spot of rust on it, which spot he afterwards observed on the chisel when first exhibited to him by the authorities—but at the time of the trial that spot of rust was no longer there. The prisoner left Beith in the afternoon of the 27th by railway for Johnstone, but stopped at Lochwinnoch, and on the following day, Sunday the 28th, he was apprehended on a charge of robbing on that day the toll-house at Lochwinnoch—a sum of money being found on him corresponding to that missing at the toll-house. The judicial declarations made by the prisoner were altogether at variance with the distinct testimony of numerous witnesses on the trial; he denied that he had disposed of a watch, and declined to explain where he got the money which he acknowledged he had on the day the body of the deceased was found; he denied, also, any knowledge of his clothes having been stained with blood, and that he had attempted to wash stains of blood from them. Besides the testimony of the witnesses who had seen the accused before his apprehension, and of Dr Haldane and a washerwoman who had inspected them after his apprehension, a minute report as to the presence of blood-stains was presented from Professor Rainy of Glasgow. The chief points brought out in exculpation were, that the watch was pawned in his own name; that the guard-chain known to have been attached to it was nowhere discovered; the assertion by two females, that there were blood-stains on the prisoner’s clothes three weeks before the murder, and that they had assisted in removing them with whiting; the testimony of a boy, that he had, in the beginning of May, given a cast in his cart at the time it contained slaughtered calves, to a man he could almost swear was the prisoner; and the testimony of a boy and a man, that they had seen a person who was not the prisoner, at nine o’clock on the morning of the murder, about three miles from the place where the body was found, with blood upon his vest—the man did not see the blood, but said that the boy spoke to him of it at the time—they did not then know of the murder; they saw him twice within twenty minutes, first going towards the place of the murder, and then in the direction from that place. The jury unanimously found a verdict of guilty, and, after sentence of death had been pronounced, the prisoner said, “I got the watch, though I never said it before, from a man named Hall, in Kilmarnock.”

If any thing were wanting to prove how perfectly circumstantial evidence, when distinctly brought out and well supported, may demonstrate guilt, this case supplies the deficiency. The common objection to circumstantial evidence as the sole proof of guilt, applies only to the manifest hazard of injustice from the effect of mere *prima facie* circumstances of guilt on the minds of an uneducated, and perhaps prejudiced jury. But where, as in the case before us, every circumstance is well attested, and all the circumstances are distinctly connected together into one whole, we confess we should prefer such proof to any ordinary amount of direct testimony unsupported by decisive circumstances. In well-supported circumstantial evidence, there is even much less hazard of being led astray by conspiracy, popular excitement, or error of identity, than in ordinary direct testimony. Direct testimony, under improbable circumstances, must be rejected; the same, unsupported by circumstances, is to be suspected; but let the circumstances be clear, connected, and sufficiently numerous, then the proof is as convincing as the proof of loaded dice from the recurrence of the same throw many times without interruption. Chance can do much, but there is a limit beyond which it cannot produce a uniform effect. In the case before us, the possession of the murdered man's watch within a few hours of the murder, and these hours the hours of the night, was a most suspicious circumstance, especially when coupled with the prisoner's solemn denial of ever having had such a watch in his possession. But he might have got the watch in the night from a person whose character he knew to be bad, and might have hoped to avoid getting into trouble by denying all knowledge of the watch; or the murder might have been the work of another from revenge without robbery, and the prisoner might have stumbled on the body in his wanderings, and taken the watch and money, and been afraid to avow what he had done. But the chisel which fitted the wound, found near the spot of the murder, was proved to have been in the prisoner's possession; it was identified by the spot of rust; but chisels being made of many sizes, even if no more than its close resemblance to that before seen in his possession, had been proved, what more could have been wanted? A man is found possessed of a murdered man's watch a few hours after the crime; he is proved to have been travelling about with an instrument, not belonging to his trade, exactly resembling the instrument of the murder, along with another weapon; when he is apprehended, the other weapon is discovered on him, the chisel is not in his possession. Can accident produce such effects as these? Can direct testimony make a case stronger than this, even when the numerous corroborating points established in this trial are left out of view? The presiding judge remarked on the zeal, industry, penetration, and efficiency displayed in procuring evidence in the case of M'Wheelan. This, doubtless, is particularly requisite in a case, in which the expectation of a conviction is to rest solely on circumstantial evidence. Here the mode of procedure in criminal matters practised in Scotland, namely, by private investigation under the authority of the Lord-Advocate, as chief public prosecutor, has peculiar advantages over the more cumbrous proceedings in England and Ireland. And it may be confidently predicted that, under the present system, convictions for crimes will become more and more difficult in these parts of the United Kingdom, simply because, as criminals become more knowing in the actual proceedings of courts of justice, the evidence of crimes will become by degrees reduced to a more and more refined evidence of circumstances. We are indebted for the particulars of M'Wheelan's trial to the *Ayr Advertiser* of October 12th, which contains a report very different from the common meagre accounts of judiciary trials, so disgraceful to our Scotch newspapers in general.

THE CHOLERA.

SINCE our last publication, the cholera has appeared in this country. The first cases occurred in Hull, and were said to have been brought by a vessel from Hamburg. On the 1st October a case was reported in Calton Street, Edinburgh;

on the 4th other cases occurred, and the disease afterwards continued to make steady, though not rapid progress. In Woolwich, two cases occurred on the 7th, and three more on the 8th instant; almost immediately afterwards the disease was announced in various parts of London, chiefly in the neighbourhood of the Thames. Uxbridge was affected on the 13th; Gravesend on the 15th; Birmingham on the 16th; and new cases continued to appear in the metropolis and in Woolwich. We must leave it to the newspapers to detail the number of cases in each of these localities. The last information which we have received indicates a renewed outbreak at Hull, and the occurrence of cases at Sunderland, York, Plymouth, Ware, the Isle of Portland, Bedford, and other places in England. The authority for several of these, however, is not stated. In London there has been for some days a diminished number of cases; in Edinburgh, also, there was for a few days a lull, but for two days past increased numbers have been again reported. The total number ascertained is already above 200. The fatality of the disease has been every where very great.

These circumstances have somewhat eclipsed the interest attaching to the news of the progress of the epidemic in other countries. We learn, however, that its virulence has declined at Alexandria, Cairo, and St Petersburg. On the other hand, it has broken out in a severe form at Smyrna, and other places in Asia Minor. In Europe it has attacked Lubeck, Amsterdam, and Königsberg, and continues to rage at Hamburg and Berlin. A case was declared in Paris, but this was officially contradicted.

We may now, therefore, consider this disease as certainly settled among us in an epidemic form; and it becomes the medical profession, as the real guardians of the public health in this country, to consider by what resources, and in what frame of mind it is to be met, and, as far as possible, averted. The weekly medical and daily non-medical journals teem with the most opposite opinions, in reading which it is impossible to avoid the humiliating conclusion, that, in regard to many most important practical points, we are in a state of ignorance quite as great as during the former epidemic. We think it would be well if medical men, instead of submitting to the public their hasty theories and crude experience on this subject, *as individuals*, would, at this juncture, try to discover those principles which result from the united experience of all, as regards the former epidemic.

In some points of view, we think we may fairly say that there is an improved condition of the public mind since 1832. The vague but intense alarm which preceded the former visitation of the epidemic, has been exchanged for a more calm and rational view of the real amount of danger. We are far from coinciding with the spirit of one of the daily journals, which, in a series of leading articles, of a characteristically impulsive and reckless character, peculiarly calculated *ad captandum vulgus*, has attempted to over-ride at once facts and public opinion, and to get up an impression that the cholera is really a disease of very slight danger, as compared with other epidemic visitations. We think, however, that we are well rid of that species of alarm, which, during the last epidemic was partly, perhaps, fostered by the absolute ignorance, even of medical men, as to the phenomena of the disease, and which frequently led to the mistake, in imagination, of the slightest and most trivial disorders for the dreaded malady.

There is no doubt that one of the causes of this improved state of public feeling, is the extent to which the opinion has been gaining ground, that the cholera is only slightly, if at all contagious. That this opinion is scientifically well founded, we have some months since expressed our strong conviction (No. for March, p. 675); nay, we have not scrupled to maintain that the weight of evidence is in favour of the view, that cholera is not under any circumstances contagious. We return to the subject of contagion now, not with the view of re-asserting dogmatically our own opinions, still less with the intention of receding from them; but in order to consider the subject in relation to the practical duty of the medical man, when called upon to give advice in matters of

action. That the public should be guided by the common sense and practical judgment of the reasonable majority of the profession, instead of by the discrepant views and clamorous assertions of extreme parties, will, we think, be generally admitted to be highly desirable. As journalists and interpreters of the public voice, we are sincerely desirous to aid in the establishment of such general principles of action as may, without the sacrifice of individual opinions, become serviceable to the cause of union.

A scientific principle or theory is a good thing, and we like to see it strongly and unreservedly stated as the result of profound conviction. But in the face of an actual emergency, and with probabilities only, not certainties, to deal with, our actions must be modified by considerations of expediency and practicability, as well as by the possible consequences of our being in the wrong. Hence we hold that the non-contagionist and the unqualified contagionist are both grievously in error, if, in spite of the obscurities of the subject, and the manifest dictates of expediency, they are willing to push their opinions in action to the utmost limits of possibility. We should consider it inexcusable and cruel in the latter to confine his patients and their attendants, and surround them with an inviolable quarantine; but, in like manner, it would be monstrous in the former, however strong his convictions of the non-contagious nature of cholera, to allow the healthy and the diseased to sleep together, or to be placed in any other circumstances unusually fitted for the development of contagion. Both must act with a due regard to circumstances; and must not attempt to enforce unbearable restrictions, or make perilous experiments, simply to carry out an individual and a disputed opinion.

We cannot deny that, in relation to cholera, the errors which flow from the exclusive and extreme contagionist theory seem to be more pernicious than those of the opposite. The former have a tendency to generate excessive alarm, to repress the charities of private life, and to destroy the comfort both of the healthy and the sick; and by thus impairing confidence and arousing fear, to create more disease than they avert. But whether this be the case or not, it is certain that the opinions of the extreme contagionist have now ceased to be held by any influential portion of the medical public. The difference in this respect between 1848 and 1832 is very marked. At the period of the former epidemic, Dr Christison had reason to complain, on the part of the Board of Health of Edinburgh, that the members of the London Board seemed to have been selected as being "unconditional contagionists;" and the acts of that Board were such as to impress the public with a most immoderate fear of contagion. At present, on the contrary, the London and Dublin boards are composed of non-contagionists; and the instructions of the London Board are evidently pervaded by the same spirit, as that which dictated the report of the Metropolitan Sanitary Commission on this subject.

Apart from all exclusive theories, we conceive that the practical part of the question as to cholera and contagion must be settled in a very similar manner, upon the data now before the public, by all reasonable men. Few will deny that the contagiousness of cholera must be very slight as compared with other epidemic diseases, seeing that, as an ordinary rule, the attendants of cholera patients are not attacked in any considerable proportion, unless placed in circumstances which, independently of contagion, are such as to pre-dispose strongly to the disease. On the other hand, it may be readily admitted, that there have been adduced, in individual cases, evidences of contagion so apparently strong as to make the non-contagionist pause long, before acting upon his theory to such an extent as to involve seriously the chances of human life. Indeed, so strongly do these circumstances operate along with the prejudice in favour of an error on the safe side, that neither the London nor the Dublin Board of Health, although evidently under the influence of the idea of non-contagion in their instructions to the public, has ventured to enunciate this principle without a reservation. Accordingly, the documents issued by both these Boards, but especially the former, present inconsistencies

calculated to detract very much from their usefulness. The public are told in the London document, that the fact of cholera spreading from person to person in close unventilated apartments, "is not to be considered as affecting the general principle of its non-contagious nature;" thus admitting in reality the whole principle of contagion in the very act of enunciating its opposite. The Dublin instructions, though framed entirely upon the non-contagionist view, admit that the disease may be "rarely" contagious.

The document containing the resolutions of the Edinburgh College of Physicians, has avoided the error of inconsistency by the simple method of maintaining a neutral position on the abstract question of contagion. It is indeed evident that, practically, the difference is insignificant between a non-contagionist *with a reservation*, and a rational and moderate contagionist; and that, therefore, no practical end would have been gained by engaging in the dispute on the abstract principle. If it be true, as we think almost every one must now admit, that the contagion of cholera is so limited as not to form an argument for encountering the great risk incurred by the removal of patients from their homes, unless under peculiar circumstances of poverty or destitution, then we cannot see how any real difference would be effected by the fullest assertion of the non-contagionist theory, unless its reckless use were to throw the public or medical attendants off their guard as to those minor hygienic regulations which, whether contagion exist or not, are of the utmost importance to the safety of the community.

We think, therefore, that those of our body who are really anxious that their advice should be useful to the public, and whose position is such as to cause their expressions to be carefully scanned, should in the present juncture avoid the dogmatic and unnecessary assertion of extreme theoretical opinions, and content themselves with allaying exaggerated fears, and avoiding rash and dangerous neglect of those precautions which are equally requisite on either theory.

We subjoin the portions of the Edinburgh document which relate to the subject of contagion. The College declare it as their opinion:—

"That it is not advisable to remove cholera patients from their own houses, if it be possible to command advantageously the means of treating them there; but that, according to experience in the former epidemic, a large proportion of cases must occur in Edinburgh among the lowest population, inhabiting apartments in which it is impossible to treat the sick for want of proper beds, bedding, fires, and other means of heating the body, besides other necessary medical resources."

"The College of Physicians also passed an unanimous resolution, strongly approving of the establishment, as in 1832, of houses of refuge, for the removal of the healthy from localities threatened with a severe visitation of the epidemic. This measure was carried through energetically by the Edinburgh Board of Health during the first epidemic; so that, at one period, upwards of 700 persons, from infected districts, were lodged under observation in houses of refuge, but allowed to go to their usual places of work. There is no doubt, according to the opinion of the best judges, that the ravages of the disease were in that way greatly circumscribed in various localities in Edinburgh; and that no other measure contributed so much to keep the epidemic within moderate bounds. The utility of such houses of refuge must be obvious to all, whether the disease be viewed as originating in infection, or, as seems now far more probable, in a peculiar miasma, prevailing with intensity only in limited localities."

SANITARY PRECAUTIONS AGAINST CHOLERA.

IMMEDIATELY after the passing of the Nuisances' Removal Act on the 4th September, it was put in force for the period of six months by an Order in Council. Some delay, however, took place in the formation of a Board of Health, which was not finally constituted till the 22d September. The General Board of Health for London is composed of the Earl of Carlisle (late Lord Morpeth),

chairman, with Lord Ashley and Mr Edwin Chadwick, to whom has been added, under the temporary act, Dr Southwood Smith as a medical member. This constitution of the Board of Health, by which a medical member is included as it were by accident, and only in the time of an epidemic, has been severely and most justly criticised. We are by no means disposed to claim for the medical profession too much power or influence as regards sanitary reform; but where a central Board of Health for the kingdom is constituted, without the admission of some of the profession as permanent members of that board, we think that the public interest must inevitably suffer, not only by the mistakes of men in office, but by the indirect action of such a board on the temper of the profession. Medical men, so far from being in general over-anxious to increase their influence by occupying a prominent position among public bodies in the work of sanitary reform, have in our opinion been neglectful to an undue extent of their legitimate influence, and have exposed themselves, not unjustly, to the charge of apathy and criminal neglect. While we admit and lament their deficiency in this respect, we must say in palliation, that when a central hygienic body can be constituted without being necessarily guided by medical opinion, or recognizing sanitary matters as subjects on which medical opinion is indispensable, it is too much to expect that practitioners shall withdraw themselves from the line of their more strictly professional labours, to study a subject in which they can have no interest but that of an enlightened patriotism.

We are happy to find that, notwithstanding these defects of the central legislation, the civic authorities of the city of London have shown a disposition actively to assist in the good work; and that a medical officer for the city has been appointed, in the person of Mr John Simon.

So soon as the existence of cholera in this country was communicated to London, the Board of Health sent Dr Sutherland and Mr Grainger to Hull to investigate the cases which had arisen there. On the breaking out of the disease in Edinburgh, Dr Sutherland was commissioned to proceed hither, while Mr Grainger went to Hamburg to report upon the epidemic on the Continent. A Board of Health was soon nominated in Edinburgh, which proceeded immediately to act, and would have continued to do so, had it not been discovered that, by the provisions of the act, the General Board had no authority to delegate its powers except to the parochial boards, and other already existing bodies. It is to be regretted that, in this way, much valuable time was lost, so far as the public interest was concerned. Instructions, however, have recently been transmitted to the parochial boards, of which it is to be hoped they will energetically avail themselves. In the mean time, we regret to learn that the excellent proposition, originating with the Sheriff and Lord Provost of Edinburgh, to combine the different parochial boards into a single working body for the benefit of the city at large, has been frustrated in the present case, as in that of the fever last year, by the determination of the West Church parochial authorities to stand aloof. Considering that the West Church parish embraces so large a portion of Edinburgh, we cannot too much regret the danger which thus arises, that some of its poorer inhabitants may, when afflicted with cholera, and requiring immediate relief, be at a loss to what quarter to proceed. We must also regard this unfortunate occurrence as a strong argument in favour of a consolidated system of sanitary administration for our large towns, instead of the present mode of parochial supervision.

We are happy to learn, however, that under the police authorities, both of Edinburgh and Glasgow, measures have been organized under the new police act, which, though late of being put in operation, may yet have the effect of resisting the progress, and diminishing the fatality, of the epidemic.

A quarantine, which was for a time imposed on the shipping in the port of London, has been abolished since the 17th inst. by an order of the Privy Council.

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No. 80. NEW SERIES.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Observations on Gunshot Wounds, made in Paris during the Summer of 1848.* By EDWARD WATERS, M.D., F.R.C.P., Edinburgh, formerly Senior President of the Royal Medical Society of Edinburgh.—(Continued from p. 316.)

TREATMENT OF FRACTURES OF THE EXTREMITIES IN CONNEXION
WITH GUNSHOT WOUNDS.

c.—Cases demanding Amputation.

IN nearly all times, civil and military surgeons have been at variance as to the necessity of amputation in certain cases of gunshot wounds, the former maintaining that many limbs could be saved which were ordinarily sacrificed; the latter, on the contrary, holding that such attempts could only be made at a great sacrifice of life. The vast experience afforded by the wars of the Empire; the writings of Larrey, Percy, Guthrie, and a host of military and naval surgeons of acknowledged merit; the prominent positions many of these surgeons have occupied as teachers during the long peace that has succeeded, rendered their opinions dominant in the schools; theoretical conclusions were obliged to give place to facts. Dupuytren, whose opportunities for observing these wounds, after the attack of Paris by the allied armies, after the days of July 1830 and June 1832, were very extensive, ended by inculcating in his lectures the same rules of action.

Since the revolution of February, and the combat of June 1848, the question has been re-opened; surgeons advocating the attempt to preserve limbs which, in accordance with the rules alluded to, should be condemned, have again come forward. On the other

hand, the disciples of what may be termed the military school, have had their confidence in their practice confirmed by the results of their recent treatment. The facts on which the former ground their arguments are not yet fairly available; the results of their attempts, except in as far as regards the mortality which has hitherto attended them, cannot now be ascertained. How far the limbs preserved may be useful, or a source of annoyance and torment, it is impossible at present to decide.

An appreciation of the merits of the two modes of practice, founded on an observation of the wounded in the late events, while the results are thus incomplete, is necessarily open to the charge of being premature and imperfect; still not to have formed some estimate of their relative value, after an attention of so long a period given to their study, would have been impossible. The development of the conclusions arrived at will now be entered on, and a few cases cited in illustration; a detail of the numerous facts observed, which bear upon the subject, would extend beyond the limits assigned to this communication. It is premised that it is not intended to establish any absolute rules of proceeding; circumstances, which the tact of the surgeon can alone estimate, may justify an exceptional course in particular cases.

M. Jobert insists on the attempt to preserve wounded limbs in all cases of fracture, where, in addition to the injury to the bone, the disorganization of the soft parts is not such as to render it hopeless in cases even where articulations have been penetrated. M. Malgaigne inculcates the same practice, with the exception that he considers amputation indispensable where the hip or knee joint is opened. The practice of MM. Ph. Boyer, Gosselin, and Laugier, has been governed by the same conservative principles. M. Velpeau, by his powerful authority, encourages to a certain extent the like practice. He stated before the Academy: "*The more I see the less I amputate*"—(*Plus je vois, moins j'ampute*). In February, he amputated less than he had previously done, in June less than in February; and he believes that, should similar cases again present themselves, he would amputate still less than in June.

On the other side, the long experience of M. Roux has convinced him that the safety of the patient requires, in regard to amputation, the adoption of the practice of the military school, and many eminent surgeons agree in entertaining the same opinion. Their conclusions possess greater weight from their having witnessed and compared the results of the two methods of practice. MM. Roux, Nélaton, and Baudens essayed the preservation of the fractured limb in several cases, while the exclusive supporters of the conservative practice pursued only the one method.

In considering the cases which demand amputation, the necessity of separating the lesions of the upper and inferior extremities is too obvious to need comment, and the examination will be in a still

greater degree facilitated by treating the lesions according to the parts implicated.

Wounds of the Hip-joint.—M. Malgaigne, whose disapprobation of primary operations has been already noticed, considers penetrating wounds of the hip-joint a case for disarticulation. M. Baudens has stated, that if a lesion of the head or neck of the femur were such as to permit him to choose between excision and disarticulation, he would decide for the former. No such opportunity has presented itself, the operation does not appear to have been ever tried in connexion with gunshot wounds; and, so long as its advantages are thus unproved, disarticulation must be laid down as the general rule in all cases of comminuted fracture of the femur at too high a point to admit of amputation. The high mortality of an operation is no valid objection to its performance when it forms the only resource of the surgeon.

Four cases of disarticulation at the hip-joints have resulted from the events of June; one of the patients, an insurgent, aged twenty-nine, operated by M. Nélaton, has survived. Of the other cases, M. Baudens performed one on the person of a young soldier; he never completely recovered the shock caused by it, and died the second day;¹—another occurred in the service of M. Richet at the Tuileries; the patient survived ten hours. The remaining case presented itself at the Hôpital Beaujon, in the service of M. Robert; the patient died during the operation. The particulars of the observation were communicated to the Academy as bearing upon the question of death under the influence of chloroform. This agent was administered in all the cases.

It might be argued that, in at least one of these instances, death was not attributable to the operation, but, however high the mortality, it is more than equalled by that which followed the attempts at conservation; one case died in the service of M. Nélaton whilst he was performing the operation of disarticulation recorded, and all the cases of comminuted fracture of the femur, involving its neck or the situation of the trochanters, which have fallen under the writer's observation, have sooner or later had the same termination.

The following remarkable case is not considered as belonging to the category absolutely demanding disarticulation:—

Joyerot, aged thirty-seven, pavier, married, admitted into the *Ambulance de la Salle des Concerts*, under M. Blanchet, was transferred, July 11, to the service of M. Malgaigne. A musket-ball had penetrated the thigh posteriorly at the fold of the nates, and issued anteriorly an inch and a half below Poupert's ligament, fracturing the neck of the femur in its course. No direct examination of the fracture was made; but it appears to have been simple. It was treated by the double inclined plane; no splinters had been discharged nor

¹ The application of ice after capital amputations seems to be contra-indicated. Its remarkable sedative effect on the pulse has been already mentioned; and it is easy to conceive that, when added to the shock which follows these operations, it may in many instances exert a very prejudicial influence.

removed, and on admission into the wards of M. Malgaigne, now twenty days after the infliction of the wound, both orifices were healed, and the fracture united by provisional callus. As shortening to the extent of nearly an inch existed, the long external splint of Desault was applied to exert extension. No action supervened, and he has been allowed to leave the hospital.

Severe lesions of the femur, in the immediate neighbourhood of the articulation, if not attended with solution of continuity, do not call for the immediate sacrifice of the limb. The following cases support this conclusion :—

Housset, *garde mobile*, aged nineteen, of good constitution, was struck by a musket-ball on the 23d June, immediately below the great trochanter, on the external surface of the femur; the ball passed into the substance of the bone to a considerable depth in a somewhat backward direction, causing a fissure of some inches in length, extending from the great trochanter obliquely downwards, inwards and forwards, and nearly, but not completely, causing actual solution of continuity. A military surgeon attempted the extraction of the ball in an *ambulance* without success. The following day the patient was admitted into the Hôpital de la Clinique under M. Giralès; inflammation had then set in, and the position of the ball could not be ascertained. Thinking it might be lodged in the soft parts, where its presence would disclose itself at a later period, the exploration was not persisted in; warm poultices were applied, and the limb laid on a double inclined plane. *October 3.* Progress to cure unsatisfactory; callus has formed to some extent, but the external wound remains open, and suppuration continues; some small portions of necrosed bone have been latterly removed. M. Giralès placed the patient under the influence of chloroform, and effected the extraction of the ball with the lever after dilating the external wound, breaking up a portion of the callus, and removing several necrosed splinters. The operation was a tedious and difficult one, and strict injunctions were given to watch the patient, and combat actively any symptoms of inflammation which might show themselves in the neighbouring joint. None such arose; the removal of the foreign bodies was a source of immediate relief to the patient, and the next morning he was reading tranquilly, and expressed himself free from suffering.

These particulars show the benefit conferred by the removal of extraneous substances, even when necessitating a severe operation. The following case presented itself in the service of M. Baudens :—

Meusy, officer in the army, wounded in the fight of June, had the great trochanter shattered by a musket-ball; all the splinters were immediately removed, and the case has progressed uninterruptedly towards cure.

Guthrie considers that amputation may be delayed with advantage in fracture of the lower part of the thigh-bone, and states that recovery not unfrequently follows. Schmucker, so favourably referred to by the late Professor Thomson of Edinburgh and Mr S. Cooper, recommended the same course. The injury produced by a musket-ball is here, owing to the less compact structure of the bone, small, compared with that which is generally produced when the central part of the shaft is implicated; and the difficulties opposed to the co-aptation of the fragments are far less than when the fracture exists in the neighbourhood of the trochanters, where the bone presents an analogous structure. M. Baudens, however, maintains the necessity of primary amputation in this as in all other

cases of fracture of the femur. It may reasonably be concluded that neither course should be exclusively followed; that the surgeon should be guided by the amount of injury, by the number and size of the splinters, and consequent loss of substance. When these are not too considerable, success may crown the effort to save the limb; in the contrary case amputation is demanded.

A printer, aged nineteen, of good constitution, admitted into the service of M. Nélaton, June 24. A musket-ball had traversed the thigh at its inferior fourth, and fractured the femur in its passage. The splinters of bone were immediately extracted, the loss of substance was not great, and slight dilatation of the orifice of entry was alone required. The apparatus of Desault, modified according to Boyer, was applied, without the too frequent accompaniments of compresses, many-tailed bandage, and cushions; the splints were slightly padded, and the limb was left visible throughout its whole length. Slight effusion occurred within the knee-joint, which was speedily absorbed. *September 25.* Consolidation of the fracture complete, a large amount of callus formed, the orifices of the wound healed, and the patient free from all uneasy feeling.

This case affords an example of the benefit of the immediate removal of all splinters; the dilatation so much dreaded by M. Jobert caused no ill consequences.

Leforestier, aged thirty-three, *chef de bureau*, of good constitution, unmarried, wounded the 24th of June, and admitted the same day into the service of M. Giraldès. The projectile, fired from a musket, had entered the thigh anteriorly between four and five inches above the knee-joint, and fractured the femur in this situation; the posterior tegumentary surface presented an opening by which a rolled mass of lead, about an inch and a half long, was removed; this orifice was supposed to have been made by the exit of a separated portion of the projectile, or by a splinter of bone. The primary splinters were extracted during the first days, and for a fortnight the limb was put up in the ordinary Parisian apparatus of three splints, cushions, many-tailed bandage, and compresses; the dressings were removed every day, and for some time twice a day. M. Giraldès then applied the gutta percha moulded to the limb, a valvular opening being practised, corresponding to the posterior orifice for the evacuation of the discharges.¹ *October 2.* Orifice of entry healed, that of exit still open, and giving issue to a slight amount of suppuration. Union of the fracture is effected by the deposition of a large amount of callus. M. Giraldès anticipates a perfect cure in this case, after the elimination of some splinters which still exist. There is shortening of the limb to the extent of fully $1\frac{1}{4}$ inch. The patient is free from fever, eats and sleeps well. He states that, when five years old, his leg was fractured at the same place, but that the cure was completed without the slightest deformity.

The conduct to be pursued in comminuted fracture by gunshot wound of that portion of the femur where the medullary canal is fully developed, and where the bony substance is remarkable for its exceedingly dense structure, is a question of the gravest interest, and continues, at the present day, to be matter of controversy. The solution of this question must depend mainly on whether, after such an accident, the realization of a useful limb be possible, and, if possible, on the rate of mortality by which such cases are pur-

¹ After a trial of some days of the ordinary apparatus, M. Giraldès adopted very generally the employment of the gutta percha.

chased. It is necessary that the limbs saved should be at least as useful as artificial ones; but it is not to be required that the amount of life sacrificed to their preservation should be as low as that which follows amputation. Where a patient has to choose between a dangerous operation, involving the positive loss of a limb, and a somewhat greater danger, with the hope of retaining it, there can be little doubt that he would prefer the latter. In computing the comparative success of the conservative and amputating practice, it may therefore be justly urged, that greater weight should be assigned to cases where, in addition to life, a useful limb is preserved, than to those where life alone is saved, and the limb amputated. If, however, it should appear either that the ratio of deaths, which follows the attempt to cure this kind of fracture, is disproportionately greater than that which follows the sacrifice of the limb; or if, in the cases where life is not lost in the attempt, the preserved limb is very rarely found to be other than a cause of embarrassment and uneasiness, amputations must then be held to be as a general rule imperative.

Gunshot fracture of the portion of the femur under consideration, is almost always accompanied by large splinters, and destruction of a considerable portion of the bone; in many cases, fissures extending into one or other of the articulations have been observed in connexion with them. Under these circumstances, consolidation cannot be otherwise than an uncommon event, and a considerable proportion of the cases in which it is sought for, are found to terminate fatally. These facts are not denied by those who endeavour to obtain it; M. Malgaigne freely admits them, and his defence of the treatment is in great measure, if not entirely, founded on the still greater mortality of the opposite method. He himself once professed this method, and put it in practice on a vast field of battle; but the result was far from answering his expectations, and, in the campaign of Poland, he lost all his cases of amputation for fracture of the thigh by gunshot wound.¹ This astonishing want of success is unparalleled in the annals of surgery, and, in consequence, is not entitled to the weight he would attribute to it; the cases should be merged in the statistics collected in reference to the same subject, and their ominous character would then be in some degree neutralized. These statistics however, as hitherto studied, do not possess an encouraging character for the operating surgeon; the examination made by M. Malgaigne of the mortality of amputation of the thigh for traumatic lesions in the Parisian hospitals, during a period of ten years, extending from 1836 to 1846, gives 24 deaths for 34 operations, somewhat less than three-fourths,² or 1 in 1.4.

¹ Meeting of the Acad. de Méd. le 8 Août 1848.

² At the same meeting, M. Malgaigne erroneously set down this mortality at more than 3.4ths.

Dr Fenwick, in his memoir on surgical operations,¹ containing incomparably the best statistics hitherto published on amputations, found the average mortality on a total of 869 amputations of the thigh for various causes, to be 1 in 2·31.

In order to throw light on the long controverted point, whether the result of amputations on account of traumatic lesions or of disease be the more favourable, he extended his researches, and succeeded in ascertaining that the mortality is much greater in the former than in the latter class of cases. In 487 amputations of the thigh, he showed that 313 performed for pathological causes gave 124 deaths, or 1 in 2·52, while 174 for traumatic lesions gave 112 deaths, or 1 in 1·55; a mortality somewhat less than that of M. Malgaigne, but still sufficiently high to shake the courage of the boldest surgeon.

M. Jobert, in his work on gunshot wounds,² after the detail of some cases of fracture of the femur, where the limb was preserved, states,—"It results from these facts, sufficiently numerous, that in every case of comminuted fracture of the bone by a ball, in an adult of great muscular power, I should be induced, in conscience, to amputate, the rather because the wounded almost always sink during the acute period, or under the abundance of the suppuration. Besides, if they resist these formidable complications, it is to retain an unserviceable member, a source of suffering without end." The views of the author have undergone a complete revolution since the publication of the above work; his only exposition of the causes of this change is that laid before the Academy of Medicine.³ From this it would appear that the mortality of the amputations, and an opinion that the sad results which attended the conservative practice were due to a mode of treatment at least as dangerous as the wounds themselves, decided him to attempt the union of all fractures; the latter reason is probably the secret of his innovations in the treatment of gunshot wounds.

Eight cases of comminuted fracture of the femur from gunshot wounds, received during the days of June, have been treated in the service of M. Jobert. At the meeting of the Academy of Medicine, on the 26th of September, he stated that six of the patients were dead; and that in the remaining two the union of the fractures was accomplished by the definitive callus. The following is a summary detail of these cases:—

Descan, aged fifteen, carter, admitted June 27, 1848; ball entered on the outer surface of the thigh, about its middle, fractured the femur, and presented itself beneath the skin at the inner side, on a plane slightly higher than

¹ Report of the Surgical Operations performed at the Newcastle-upon-Tyne Infirmary, during a period of seventeen years and a half.—*Monthly Journal of Medical Science*, Nos. from October 1847 to February 1848, inclusive. Translated in the *Arch. Gén. le Méd.* 1848.

² *Des Plaies d'Armes à Feu*. 8vo. Paris, 1833.

³ Meeting of September 26, 1848.

that of the orifice of entry, whence it was extracted by incision. No dilatation of the wound nor extraction of splinters was performed; cold poultices were applied during nineteen days. July 29.—Two days since, patient was removed from the ward *Napoleon* to the ward *St Augustin*; his appetite and sleep have been slightly troubled in consequence; appetite good previously. Considerable swelling of the thigh, heat natural, suppuration moderate, no fever. Sept. 25. (Period when the case was reported to the Academy.)—Internal orifice healed, external still open; wounded thigh fully twice as large as the other, no tension, suppuration diminished; large deposition of provisional callus, irregular in form; shortening of the limb to the extent of two centimetres. The patient is completely free from suffering. Oct. 16.—Swelling of fractured thigh somewhat diminished; suppuration by external wound continues, but is slight in quantity. Extension by pelvic and foot bands is still maintained; the pelvic band is passed beneath the groin on the unaffected side. Oct. 24.—Suppuration still continues.

Philippe Hubert, aged twenty-six, blacksmith, unmarried, admitted June 23, 1848; wounded by a musket-ball, which traversed the thigh, fracturing the femur somewhat below its superior third. No dilatation of the wound, nor examination of the fractured bone, made; cold poultices applied. Swelling and tension great, to relieve which M. Jobert practised two longitudinal incisions, unconnected with the tract of the ball. An abscess subsequently formed, and was opened about the third week. The cold poultices were continued for about three weeks. Sept. 23. (Period when the case was reported to the Academy.)—Free from suffering, general health good; position of the limb good, extension kept up by pelvic and foot bands, as in the last case; another band extended across the bed, and, attached to its sides, prevented elevation and displacement of the knee; no splint in either case from the commencement. Oct. 16.—Limb straight, shortening to the extent of two centimetres (about three-quarters of an inch); suppuration continues, pain experienced in neighbourhood of fracture; general health not so satisfactory, some irritability, and diarrhoea. Oct. 24.—Abscess at the inner side of the thigh healed; general health good; fistulous opening still exists. Warm baths.

M. Malgaigne, in the report of his wounded before the Academy (August 8), stated that he had received five fractures of the thigh, and that of these two were cured. One of the successful cases was treated during the first three weeks by M. Blanchet, and was only admitted into the service of M. Malgaigne, July 11. This case cannot, therefore, be correctly included in the statistics of the latter.¹ Of the other cases treated by him, the following alone survives.

Guillaumet, *garde mobile*, aged twenty-two, wounded June 24; the ball entered the thigh anteriorly, about the middle of the rectus muscle, passed backwards and slightly internally, fracturing the femur in its course. The bone would appear to have been struck obliquely on its inner side. No examination into the state of the fracture was made. Sept. 24.—The patient states that, from the time of admission, he has been exempt from suffering; that, at the beginning of August, the posterior orifice was closed, and that the anterior one likewise healed towards the end of the same month, without a single splinter having passed out or been extracted. Consolidation of the fracture is effected with riding of the fractured extremities, the thigh being shortened to the extent of more than an inch, and presenting a curve with the convexity outwards. Oct. 24.—Has continued perfectly free from pain. At

¹ This case is already recorded in connexion with disarticulation at the hip-joint.

the visit of this morning, M. Malgaigne declared this to have been a case of simple fracture of the femur.

The following is the only case of fracture of the shaft of the femur under treatment in the wards of M. Velpeau :—

Leroy, aged twenty-three, soldier, admitted the 25th of June, with fracture of the femur, somewhat above its middle, by a musket-ball. No extraction of splinters was practised. Sept. 26.—Fractured thigh considerably swollen, but without tension; suppuration continues; provisional callus in large quantity, and irregular form. Oct. 31.—There appears but little prospect of the limb being a useful one, if even it be saved; there is considerable swelling, with discharge of thin dark-coloured pus, together with marked deformity, the thigh being curved outwards.

Three cases of comminuted fracture were treated by M. Laugier. One of these cases, which has terminated fatally, is excluded, as it would have necessitated disarticulation at the hip-joint. Of the two remaining, one is also dead, the other is still under treatment :—

This patient, a washerwoman, aged thirty-six, married, of excellent health, active and steady habits, was wounded accidentally, June 25, by a musket-ball, which traversed the thigh, shattering the femur about its centre. No dilatation nor extraction of splinters was performed; but the presence of numerous fragments was distinctly perceptible during the pressure exerted for the discharge of the pus, at the time of changing the dressings. The ordinary apparatus of compresses, many-tailed bandage, junks, splints, and "*porte-attelles*," was applied, but no extension kept up. Nothing but the excellence of her constitution seems to have supported this woman through the course of prolonged suppuration, fever, and occasional attacks of diarrhoea, which she has undergone. She is now much emaciated; suppuration continues; there is considerable deformity of the limb, and consolidation is incomplete, splinters in all probability still remaining in connexion with the fracture.

In the service of M. Richet, at the Tuileries, which is considered to have presented the best possible hygienic conditions for the patients, three, with comminuted fractures of the femur, resisted amputation, and died respectively on the 10th, 17th, and 29th days.¹

To avoid further details, the following Tables have been drawn up. They comprise all the cases connected with the present inquiry amongst the wounded of June, of which the writer has been able to complete the history up to the present time :—

¹ M. Huguier attributed the success of his amputations at the Tuileries to the favourable hygienic conditions; the fractures in which conservation was attempted were not, on that account, the less fatal.

TABLE I.

Cases of Fracture of the Femur where Amputation in the continuity of the Bone was practicable, but in which Preservation of the Limb was attempted.

Surgeons.	Cases treated.	Deaths.	Remaining under treatment.
M. Jobert . .	7	5	2
... Malgaigne .	4	3	1
... Boyer . .	5	5	0
... Baudens . .	2	2	0
... Richet . .	3	3	0
... Giralès . .	2	1	1
... Nélaton . .	1	0	1
... Langier . .	2	1	1
Total .	26	20	6

TABLE II.

Cases of Fracture of the Femur treated by Amputation.

Surgeons.	Cases.	Deaths.	Cures.
M. Richet (Tuileries) . .	1	0	1
... Larrey	1	0	1
... Baudens	1	1	0
... Roux	1	1	0
... Blandin	1	0	1
... Monod (Maison de Santé)	1	0	1
... Huguier (Tuileries) .	3	0	3
... Gerdy	2	2	0
Total	11	4	7

These Tables do not include all the cases afforded by the events of June ; but having been collected generally, and not selected, they may be held to offer a fair criterion of the success of the two methods of treatment.

The first Table has been framed so as to give the most favourable character the materials permit to the conservative practice; it includes a case of simple fracture, the only one remaining in the service of M. Malgaigne, and the case of fracture in the lower fourth of the femur treated by M. Nélaton, where the splintered fragments were small in number and size, and removed immediately; the particulars of both these cases have been already given. It has been impossible to include the cases of fracture of the femur admitted into the wards of M. Velpeau, from the writer having failed to reconcile the facts there observed with the account of them given to the Academy; the details of the case still under treatment have

been given in illustration of what so high an authority considered, as far back as the 12th of September, a case in progress of cure.¹

What is the result of the conservative practice? Twenty-six cases have been adduced in which it was adopted; of these 20, or 77 per cent., have already succumbed. Six only survive, all of them with shortened limbs, and of these one was a case of simple fracture; in this and the case of M. Nélaton, of fracture at the lower fourth, the external wounds have healed; in all the others fistulæ exist, and suppuration continues. Further, in one of the latter cases, that of M. Giraldès, the fracture also occupied the lower fourth of the femur, the situation which Larrey, Schmucker, Guthrie, and S. Cooper conceived to justify an attempt to save the limb; even in this case the progress to cure is still complicated by the presence of a fistula and portions of necrosed bone. The case of M. Laugier presents little prospect of a favourable termination. The two remaining cases are those of M. Jobert, in which he reported the consolidation as completed by the formation of the definitive callus (*2 ont guéri par cal secondaire*) on the 26th of September, a time at which, it appears from the report given of the cases, that swelling and suppuration still existed, and that extension was still maintained. The re-union of the fractured extremities by definitive callus is a process which is known not to be completed under a twelvemonth, and, once accomplished, extension can no longer exert any influence. It cannot be sustained that, in these cases, union had proceeded thus far; on the contrary, the fistulæ and the suppuration clearly denoted the presence of fragments of dead bone interfering with the progress of the cure. It is not improbable that these cases may turn out to be such as M. Bégin alluded to when he stated: ²—"I have frequently met persons, to whom it was believed that a great service had been rendered by the preservation of their limbs, who, after many years of suffering, presented themselves to me with fistulæ, inflammatory swellings, inexhaustible suppuration, rendering amputation necessary, when it was thought that their limbs had been for ever saved. Amongst others, I remember the case of an officer wounded at Lutzen, in whom splinters of the tibia were enveloped by a bony production of new formation. Abscesses formed every year, intolerable pain supervened. I was obliged to amputate the thigh twenty-five years after

¹ In collecting the observations of a great number of cases in different hospitals, it is possible that some may have escaped notice. At the sitting of the Acad. of Med., Sept. 12th, all that M. Velpeau stated regarding his fractures of the femur was:—"Sur 5 fractures de cuisse, je n'ai fait qu'une amputation; des 4 autres, 1 est sorti; les 3 autres sont en voie de guérison."—(*Gaz. Méd. le 16 Sept., 1848*). The amputation was performed during the period of reaction, and was fatal; the case discharged could not be considered cured, its termination could not be predicted at so early a period.

² Acad. de Médecine, 26th Sept. 1848.

the infliction of the wound." M. Bandens referred to similar cases, and most military authors have adduced examples of them.

M. Jobert believes that splinters of dead bone may contribute to the permanent consolidation of the fracture; a belief opposed to the principles of surgery as taught in every age and country. Rare cases have existed, where balls have been lodged in bones without creating disturbance; but no example has hitherto been recorded of necrosed bone occupying the same position with impunity.

Twenty of those whose limbs were thus attempted to be saved, have already died; six out of twenty-six survive, and, in some of these, there is every probability that the limb must yet be sacrificed. On the other hand, Table II. gives four deaths for eleven amputations, a mortality less than one half of what has already occurred in the other series,—a result decidedly in favour of amputation in these cases. It is to be observed that the amputation of the thigh, performed by M. Baudens, was not fatal till the 34th day, when the stump was completely healed, with the exception of a small spot. The case was that of General Damesne, who suffered much anxiety as to his prospects, and the dread of losing his command. In some of the other fatal cases the thigh was amputated after the period of inflammatory action had set in, which experience has proved to be the most unfavourable period for operation.

It may be urged that the statistics here brought forward are too limited to influence the decision of the question; they, however, place beyond doubt the delusive nature of the hopes which are generally entertained during the early period of the conservative practice, the unsatisfactory nature of the results in the great majority of the successful cases, and the frightful mortality at which they are purchased. In this they only bear out the conclusions of previous observers. When Gualtiér de Claubry, formerly surgeon to the imperial guard, joined the army of Italy, he entertained sanguine hopes of success from the conservative treatment; the mortality which attended his trial of it obliged him to abandon it. With regard to the army in Spain, he states that almost all the military, who had the thigh fractured, died when immediate amputation was not performed. Paillard joined the army at Antwerp with the same conviction, and underwent the same conversion. Ravaton, Lombard, Ribes, have held the same language. According to Percy, scarcely 1 in 5 recovers of those who have suffered gunshot fracture of the femur; according to Guthrie, 1 in 6; and Schmucker, 1 in 7.

M. Malgaigne supported his views by a reference to the results of Dupuytren's practice in 1830, thus drawing an opposite conclusion from the same cases. In a clinical lecture delivered after the *émeute* of June 1832, Dupuytren stated,—“I have repeated it frequently, and I repeat it for the last time, after the facts which I have witnessed, principally in 1814, 1815, and 1830, my opinion on this point is not to be shaken. In compound fractures, in those

especially by gunshot wounds, in rejecting amputation more individuals are lost than limbs saved."¹

The most extensive scale on which the merits of the conservative practice have ever been tested, was in the Prussian army under the authority of Bilguer, who announced at the conclusion of the war, as quoted by Sir G. Ballingall, that, amidst 6000 wounded, the Prussian surgeons had not amputated a single limb. Out of 300 cases of fracture of the extremities, he cites 11 as instances of surprising cures, and even these are considered destitute of the accuracy and minuteness of detail which would render them useful. He excludes from his calculations gunshot fracture of the femur as being perfectly hopeless.

To deny the mortality which attends the preservative practice in these cases after this evidence, would be in vain; neither is it so much by denying a high mortality, as by adopting the general conclusion, that the wounded do not run a greater chance of death by seeking to preserve their fractured limbs than by amputating them, that the advocates of the attempt defend their practice. In proof of the high mortality which follows primary amputation, M. Malgaigne adduces the statistics which have been already quoted, and which give a mortality of 70 per cent. in amputation of the thigh. The statistics of Dr Fenwick, which are more extensive, and include those of M. Malgaigne, reduce the mortality to 64 per cent. Deciding the question by these numbers, the balance in favour of amputation is considerable; for, assuming the limbs preserved to be 1 in 6, and a more favourable calculation does not seem justifiable, the mortality is more than double that yielded by the statistics of Dr Fenwick.

As has been stated, the important fact, that the amputations for traumatic lesions, almost exclusively primary, performed in civil hospitals, are fatal in a much greater proportion than those performed for pathological lesions, such as diseases of the joints, in which the condition of the patient is assimilated to that existing in secondary amputations, appears established.

Is, however, the practice with respect to gunshot fractures to be governed by these statistics? The writer believes not; whereas it is by the ratio of deaths, thus afforded, that M. Malgaigne seeks to settle the mortality of amputation in these cases. He is well acquainted with certain facts, which seem to militate against such a course. Thus he cited the success of Fercoq in 1794, who performed 60 primary amputations on beaten sailors, all of which, except 2 followed by tetanus, were successful, being a mortality of 1 in 30; of Percy, who, after the affair of Neubourg, out of 92 amputations of the leg, thigh, and arm, lost 6, or 1 in 15; of Guthrie at New Orleans, who, out of 45 primary amputations, had 7 deaths,

¹ Dupuytren. *Traité des Blessures par Armes de Guerre; Leçons Recueillies et Publiées par Brierre de Boismont et Marx.* 2 vols. Paris, 1830. 2e. éd.

or 1 in 7; after the battle of Toulouse, 47 amputations, 9 deaths, or 1 in 5; of the English army in Spain, 291 amputations, 24 deaths, or 1 in 8. "Del Signore at Navarino; here it is superb; 31 primary amputations, 1 death, 1 in 30!" "The English surgeons at the battle of Aboukir, and of Camfordomer, 30 primary amputations, 30 cures!!!" Larrey during the days of the 27th and 29th *brumaire*, 1 in 6. To these he might have added, that Masselet, after the battle of Aboukir, performed 11 primary amputations, all of which were successful.

M. Malgaigne endeavours to discredit these results, not by evidence or reasoning, but by irony; his statistics do not correspond with them; the greater the success, the greater the number of marks of admiration with which the paragraph announcing it is honoured. It is not thus, nor yet by the application of the epithet "*superbe*," that facts are to be overthrown, and science advanced.

These facts exist, and, instead of denying, it should be sought to explain them; they are not more marvellous than the success of M. Huguier, who performed 19 capital operations for fractured extremities during the days of June, including several amputations of the thigh, humero-scapular disarticulations, &c., without a single death.

By the statistics collected by M. Malgaigne, of amputations performed in the Parisian hospitals for traumatic lesions from 1836 to 1842, a period free from any popular struggle, and therefore not productive of gunshot wounds, it is shown that 182 operations were followed by 117 deaths, being a mortality of 1 in 1.55, or 64 per cent.; the statistics of the Newcastle Infirmary, published by Dr Fenwick, give in 81 traumatic amputations 26 deaths, an average mortality of 1 in 3.11, or 32 per cent; the statistics collected by him from the returns of different hospitals, in 597 similar operations, 304 deaths, a mortality of 1 in 1.96, or 51 per cent.

When these results are compared with those above cited, the discrepancy is so great, as naturally to suggest the inference, that they form two distinct classes of cases—a conclusion which derives confirmation from the military statistics of Mr Alcock, which give on a total of 664 primary operations an average mortality of 1 in 8.3, or only 12 per cent.

How is this discrepancy to be explained? What is the distinction between the traumatic lesions which present themselves in civil hospitals, and the lesions met with in gunshot wounds? This distinction is believed to lie in the more serious nature of the injury, and consequent grave character of the nervous commotion, which exists in the former class of cases. The traumatic lesions necessitating amputation, admitted into civil hospitals, are ordinarily caused by railway accidents, by machinery, crushing of some of the extremities by the fall of stones, weights, &c., the passage of carriages over an extremity, and so forth, producing frequently a frightful amount of laceration and destruction of parts, and the

nervous commotion attending these accidents is proportionably severe. The effect of this is evident in the statistics of Dr Fenwick during the four first days; in other words, during the period when nervous commotion would exert its most prejudicial influence, the number of deaths is 10 times greater in amputation of the thigh, and 8 times greater in amputation of the leg, for traumatic, than in the same operation for pathological causes; while 74 pathological amputations did not occasion a single death from nervous commotion, 106 traumatic amputations gave 12 deaths from this cause alone, or 1 in 8·83; the same with gangrene of the stump, which is attributed to nervous disturbance (1 in 17·66), and which generally terminated fatally in the four first days after traumatic amputations, while it did not cause one death in those for disease.

If the frightful mortality from these causes, which traumatic amputations produce, be compared with the almost insignificant mortality of amputations practised for disease of the joints, one cannot avoid being struck with astonishment; for in the latter case, during the period of nervous commotion, there are not 2 deaths in 100 operations. These facts are placed beyond doubt by the tables of Dr Fenwick; and they show that the greater mortality of these over pathological operations, is, in chief measure, due to the shock to the system caused by the accident.

Authors have commonly insisted on the shock to the system which is caused by gunshot wounds, and on the necessity of deferring amputation till its effects are passed away. The observation of modern military surgeons goes to show that these effects have been greatly exaggerated. In the numerous cases which fell under the writer's notice, he did not witness in any of the cases of fracture of the extremities, any such amount of shock as would counter-indicate immediate operation, no marked effect on the pulse, no coldness nor clamminess of the surface, no stupor; in several cases of abdominal wound, the signs of commotion were strongly pronounced, and the contrast was most striking. Sir G. Ballingall maintains, that the patient supports an examination into the condition of the injured parts, immediately after the infliction of the injury, better than at any subsequent period. M. Baudens has stated to the writer, that in his experience it has been exceedingly rare to witness general commotion from fracture by musket-balls, unless perhaps where the bones have been shattered in the neighbourhood of the hip-joint, but that the case is different as regards the effect of cannon-balls or howitzers.

The more extensively the method of amputating on the field of battle has been tried, the more clearly have its advantages been demonstrated; M. Bégín insists that amputation should be immediately performed, in order to give the patient every advantage. The success attending it is incompatible with the supposition of the existence of shock. The well-known conversion of the French surgeons, by witnessing the success obtained from immediate opera-

tions, performed by the American surgeons during the war of independence; the excellent influence on the *morale* of the troops, which the flying *ambulances*, first organized by Larrey under Napoleon in Italy, and which were adopted for the immediate performance of amputations, exerted; the fact of 664 cases of operations performed on the field of battle, collected by Alcock, only yielding a mortality of 12 per cent, combined with the examples of success which have been quoted after M. Malgaigne; and the still more astonishing success of Dr Burke, recorded by Sir G. Ballingall, of 80 cases of amputation with the army in Upper Scinde, not one of which had terminated fatally during the first 14 days—are so many additional proofs that nervous commotion, that powerful cause of mortality in the traumatic operations in civil hospitals, is in great measure, if not entirely, wanting in cases of gunshot fractures, and explains the superiority of the success which follows amputation in these cases, over that which is observed under other circumstances. The excess of mortality in the traumatic operations in civil hospitals over that in the pathological cases, is not to be attributed to the fact of the patient being in the one instance in his ordinary state of health, in the other prepared for the operation by confinement and suffering—a conclusion to which the compilers of statistics seemed to be fast arriving; but to the mode of infliction and severity of the traumatic lesions which render the operation necessary.

Should this view be confirmed, the bearing of the statistics of amputation in gunshot wounds will be entirely changed, and surgeons will no longer be seen, appalled by their formidable character, forgetting their doctrine and theory, to temporise. Amputation will be freed from the terrors which now surround it, and its adoption, in the true interest of the wounded, be resorted to in those cases of fracture of the femur in which it has been held to be indicated.—(*To be continued.*)

ARTICLE II.—*Notes on the Epidemic Fever of 1847-48.* By WILLIAM ROBERTSON, M.D., F.R.C.P., Physician to the Edinburgh Royal Infirmary.

THE following observations upon the fever epidemic of 1847-48, are founded solely upon data furnished by my own wards in the Royal Infirmary, from 1st March 1847 to 1st March 1848, and exhibit a faithful record of the proportionate mortality in the two forms of fever lately prevalent. I say *two* forms, because, after excluding cases of Ephemera and of Ague, I met with none which could not be included under the heads of Typhus and Relapsing fever. Another head I would gladly have introduced for the classification of the cases of Dothinerterite, of which, in the early part of the epidemic, a considerable number was observed. From what

I saw, however, in the dissecting theatre, I soon became convinced that the diagnosis of this disease is uncertain; that although it is easily recognised during life in certain exquisite cases, accompanied with its most characteristic symptoms, yet in others it may totally elude observation; and, above all, that cases of our ordinary Edinburgh typhus may go on to a fatal termination, deceiving the physician into the belief that *Dothineritis* exists, and yet, on post-mortem examination, revealing only those equivocal appearances so frequent after death from fever. Besides, I am inclined to consider the so-called *dothineritis* as a mere variety of typhus, and have accordingly arranged under that head all the fatal cases which occurred to me.

For the explanation of the following table it is necessary further to state, that in a few instances convalescents from relapsing fever became affected with typhus, while convalescents from typhus contracted relapsing fever, before being dismissed from the wards. Such cases are invariably twice entered in the table; and when death ensued it is always attributed to the form of disease under which the patient last laboured. A large number of the deaths were immediately caused by dysentery, pneumonia, and similar affections, occurring after convalescence from fever seemed fairly established. I have, however, judged it proper to retain these fatal cases under the heads to which upon admission they were properly assigned, persuaded that by any other course I should present a very unfair view of the average mortality. A considerable number of miscellaneous cases find their way into fever wards—no reference is made to such cases in the table.

From the 1st March 1847 to the 1st November, the cases in Fever Hospital A (a building in Surgeon Square) were carefully collected; during the winter months my charge only extended to Shed E (a wooden building erected against the south wall of the Infirmary), and the two fever wards, 9 and 15, in the top story of the old building. The gentlemen who assisted me as resident clerks—Drs Begbie, Baikie, Morris, England, and others who acted for shorter periods—all kindly took their share in the labours necessary to make these statistics accurate, and no one who has not been engaged in similar pursuits can estimate the trouble required for such a purpose. The dry task of daily entering, dismissing, and classifying a large number of cases, is no pleasant addition to the daily and nightly duty of superintending a hundred fever patients. To these gentlemen my best thanks are due for the zeal and efficiency with which all their duties were performed.

In a communication read to the Edinburgh Medico-Chirurgical Society, in July 1847, Dr Bennett, at that time pathologist to the Royal Infirmary, stated that from 1st November 1846 to 30th June 1847, 2071 cases of fever had been treated in the hospital,

and that of this number 278 had proved fatal—i. e., there was a mortality of 13·42 per cent. Now, it is notorious that considerable errors find their way into the general statistical record of the hospital from which Dr Bennett must have drawn the above statement; yet it is singular that this rate of mortality coincides very accurately with that deducible from my own table, if the calculation be restricted to equal numbers of *male* and *female* cases.

TABLE.

ADMITTED IN	MALES.				FEMALES.			
	Typhus Fever.	Died.	Relapsing F.	Died.	Typhus Fever.	Died.	Relapsing F.	Died.
March.	38	7	17	0	20	2	13	0
April.	38	10	20	0	29	10	22	0
May.	32	2	20	1	21	2	9	0
June.	31	9	23	1	24	1	16	1
July.	23	10	24	0	13	2	35	1
August.	25	8	43	2	18	3	28	1
September.	17	2	39	8	9	2	20	0
October.	18	3	30	0	7	2	25	0
November.	29	9	60	5	13	7	23	0
December.	22	6	35	0	15	6	13	0
January.	37	15	32	1	17	3	14	1
February.	20	6	15	1	22	6	13	0
Total	330	87	358	19	208	46	231	4

Mortality of Males.	Mortality of Females.
From Typhus . . . 26·36 per 100.	From Typhus . . . 22·11 per 100.
From Relapsing Fever 5·3 per 100.	From Relapsing Fever 1·72 per 100.

Gross mortality from Typhus . . . 24·72 per cent. or 1 in 4 nearly.
 Gross mortality from Relapsing Fever 3·9 per cent. or 1 in 25·6 nearly.

Total of Fever cases 1127; of which died 156.
 Average mortality 13·842 per cent. or 1 in 7·224.

But the above calculations are founded upon 688 male cases, and only 439 female.

The average mortality in males was 15·407 per cent.
 The average mortality in females was 11·389 per cent.

The mortality in equal numbers of *male* and *female* cases, may consequently be stated at 13·398 per cent. or 1 in 7·46—numbers very closely approximating to those given by Dr Bennett.

In a paper published in the October number of the Edinburgh Medical and Surgical Journal, Dr Robert Paterson exhibits an elaborate statistical account of the cases treated by him in the sheds and tents erected last year for the accommodation of fever patients within the grounds of the Royal Infirmary. He considers

that the small mortality in the sheds and tents proves that such erections are better adapted for the treatment of fever than the ordinary fever wards of an hospital. From this conclusion I must dissent for various reasons. *Firstly*, I am sure that the female fever cases treated in the tents and sheds, were for considerable periods milder than those received into the main building of the Infirmary, and into the fever hospitals in Surgeon Square. During the months on which the duties of the admission-room devolved upon myself and my clerks, we uniformly acted upon what we deemed the *humane* principle, of not sending bad cases to the tents or sheds when there was room for them elsewhere. Dr Paterson is possibly correct in his statement, that the different forms of fever were observed in equal proportion in the tents, sheds, and ordinary wards of the hospital; but certainly he had not many patients sent to him *moribund*, nor do I think that the records of the tents can embrace many instances of death occurring within 48 hours after admission. The ordinary wards of the institution received far more than a fair proportion of unpromising and desperate cases, and to this circumstance I am inclined to attribute the large mortality observed in the house, as contrasted with that of the sheds and tents. *Secondly*, My own experience of the treatment of fever in sheds was not satisfactory. The shed E was under my charge all winter, and I had an opportunity of contrasting the progress of patients there, with that of similar or more severe cases treated at the same time in ward 9 in the wing of the main building. I well recollect the ravages which dysentery, pneumonia, and acute inflammations of the serous membranes caused among my convalescents in the shed towards the middle of winter, while the patients in No. 9 enjoyed a comparative immunity from these affections. *Thirdly*, Granting, as I most cheerfully do, that the experiment of treating fever cases in tents, or such "temples of the winds" as the Infirmary sheds, proved eminently successful during the mild dry summer of 1847, I do not think it probable that we should be equally fortunate on future occasions, unless we could calculate upon the prevalence of the epidemic during equally genial weather. I can see no solid reason for preferring a tent or shed to a well kept fever ward; and must accordingly, on public grounds, enter this *caveat* against the conclusions of Dr Paterson. At the same time, I must bear testimony to the energy and success with which he laboured at the Infirmary during the prevalence of the epidemic, and to the pleasure which I myself had in co-operating with so able and estimable a colleague.

From March to July 1847, while scurvy prevailed to a great extent among the railway labourers in the neighbourhood of Edinburgh, and among the poor of the city itself, a considerable number of fever cases, complicated with Scorbutus, were admitted into the hospital. On theoretical grounds, it seemed likely that the fever would in these cases prove of an unusually malignant type, and such certainly was its character in the two first cases of the kind which

fell under my observation. My notes do not, however, show that the gross mortality was materially affected by the *scorbutic* complication. Of 26 cases which I find recorded, 21 occurred in males, 5 in females; 2 of each died,—a mortality of 15·4 per cent. It must be recollected that the complication in almost all these cases co-existed with typhus. One died apparently from the exhaustion caused by repeated epistaxis, another in consequence of hæmorrhage from the stomach and mucous membrane of the lungs.

Numerous examples of *dothineritis* were observed in the earlier period of the epidemic, chiefly among patients brought from the country, and especially among the railway labourers on the Hawick line, to the south of Dalkeith. For reasons already assigned, I am unable to state the proportion of deaths occurring among patients who laboured under this form of disease. There is, I think, reason to fear that it must have been very large, probably (as in the experience of some Parisian physicians) amounting to nearly a third of those affected. Dr John Reid, while pathologist in the Royal Infirmary, used to remark the extreme rarity of *dothineritis* among the inhabitants of Edinburgh; and, in confirmation of the accuracy of his observation, I can state that no case of *dothineritis*, authenticated by post-mortem examination, and occurring in an inhabitant of Edinburgh, has for the last three years been seen in my wards.

Physicians have been long familiar with the enlarged and softened state of the spleen, so commonly found after fatal attacks of typhus. During the late epidemic, another morbid state of the viscus was very frequently observed, consisting of deposits in its substance of a brownish grey colour, and of a cheesy or creamy consistence.* These deposits were of various sizes, from that of a walnut to that of a pin-head—there were often several in the same spleen—and they were most usually situated immediately beneath the peritoneal tissue of the organ. They were most frequently noticed after death from typhus, but were not uniformly, and I think not even commonly, associated with *dothineritis*—in at least one case they were observed after death from relapsing fever. Although I made many examinations of these deposits with the microscope, I never could convince myself that they were histologically identical with the so-called *typhus deposits* found in the glands of the ileum and mesentery. For several months past, few or none of these exudations have been seen in the bodies of fever patients examined in the Infirmary. In the summer of 1847, I lost two patients from *peritonitis*, consequent upon the rupture of softened splenic deposits into the sac of the peritonæum.

Observations on the Relapsing Fever.

The relapse on the 14th day seems to be the most regular feature

* Monthly Journal, vol. ii. p. 200.

of this disease. Various attempts were made to check its periodicity, by the administration of different remedies during the remission. Quinine, Bibberine, Arsenic, and Digitalis, were all fairly tried, and all proved equally inefficacious. The only practice which seemed occasionally to postpone the relapse, or to lessen its violence, was the administration of a powerful emetic early on the 14th day of the disease. Several of my hospital patients who were subjected to this treatment left the house about the 17th day, believing themselves cured; others did not relapse till the 18th day, and I believe that relapses may have occurred among those who were dismissed at their own desire, persuaded that the disease would not return. One of my resident clerks (Mr Rennie), who had a severe attack of this form of fever, took the emetic on the 14th day, and did not relapse till the 18th.

Jaundice was less frequent than in the epidemic of 1843. When present, it was usually accompanied with tumefaction of the liver, which was sometimes by percussion ascertained to extend from 3 to 4 inches below the right costal cartilages. The increase in the dimensions of the liver was also frequently observed in patients who were not jaundiced. It was interesting to watch the rapidity with which this *portal* (?) engorgement subsided under the use of appropriate remedies. Saline purgatives frequently repeated, and the detraction of a few ounces of blood from the epigastrium by cupping or leeching, generally reduced the viscus to its normal dimensions in a few days. I seldom had recourse to blistering in these cases, and never gave mercurials, except calomel, as a *purgative*.

The spleen, if the indications of percussion may be trusted, became much more frequently engorged than the liver. In almost every case in which I examined the left hypochondrium during the height of the disease, the dimensions of the spleen considerably exceeded the normal,—the dulness in extreme cases extending over a space of fully six inches by five. It was remarked that the margin of the enlarged spleen could seldom be detected below the cartilages of the ribs,—its upper border was sometimes ascertained to correspond with the fifth intercostal space. There was very rarely pain or tenderness over the dull region. This affection of the spleen usually soon subsided after the sweating crisis; in some cases, however, it did not yield without treatment. The most efficient remedies seemed to be combinations of aloes with Sulph. Quinæ or Sulph. Ferri, and the Syrup. Ferri Iodureti.

In five or six cases, a remarkable form of delirium was observed immediately to precede the sweating crisis. Its peculiar characters were its suddenness, its violence, and its extremely transitory nature. In the first case of the kind which I witnessed, the patient was labouring under a *relapse* of average severity, and had conversed with me quite coherently when I passed his bed at the daily visit. Scarcely had I left the ward, when I was recalled to assist this patient, who had suddenly become most outrageous, screaming

aloud, raving, and abusing the attendants who were endeavouring to prevent him from jumping out of bed—stools and urine were at the same time passed involuntarily. Within fifteen minutes the same patient was lying calm and collected, forgetful of what had passed, and bathed in a most profuse and critical perspiration. After I had noticed similar phenomena in three or four cases, I was informed by my clerk, Dr Heslop, that these curious symptoms were of more frequent occurrence in the relapsing fever of Dublin, and that they had already engaged the attention of some of the physicians there. To the best of my knowledge, they have not been hitherto described in connexion with the Edinburgh epidemic.

ART. III.—*Case of Wound of the Chest, penetrating the Right Ventricle of the Heart.* By JOHN TRAILL, Surgeon, Arbroath.

(Communicated by Professor Syme.)

ON Wednesday evening (September 20th) I was hurriedly summoned to Kinblethmont House, about four miles distant from Arbroath, to visit Alexander Gray, stable-boy to W. F. L. Carnegie, Esq., who was reported to have been stabbed in the side, and to be in a dying state. On reaching Kinblethmont, I found the lad (nineteen years of age, and stout and healthy) in bed, to which he had just been removed; he was very pale, with a strong expression of anxiety in his countenance—the whole surface of his body cold and damp—and his pulse so feeble as to be scarcely perceptible, and very quick—his breathing was natural and easy, and he said he had not much pain. He was vomiting, and had previously vomited a large quantity of vegetable matter in a fermented state. On the left side of the chest I found a punctured wound, commencing at the lower edge of the third rib, about an inch and a half from the sternum, and penetrating obliquely downwards and forwards to the depth of two inches and a half—the probe touching the fourth rib, and then passing onwards to the sternum, so that it could be felt under the integuments nearly in the mesial line.

I now learned that the boy had been in Arbroath at a horticultural show, where he had ate a large quantity of apples, plums, and other fruit, and had also drunk several glasses of rum. On coming home, being somewhat excited, he had gone into the laundry where one of the housemaids was seated sewing, and, by way of teasing her, had gone behind the chair, and, kneeling or squatting on the floor, had suddenly seized her by the elbows and endeavoured to pin her back. The girl (a stout young woman) had at the time a boil on her arm, and the lad unfortunately seized the arm over the sore, so as to cause great pain; this made her start up violently from the chair, at the same time striking back with the disengaged arm (the left), in the endeavour to drive him away. At the moment she chanced to have in her hand a pair of sharp-pointed scissors, and, quite forgetting this, the scissors were forcibly struck against the chest so as to produce the wound above described.

The boy immediately got up from the floor and rushed into the servants' hall, where he sunk down upon a chair, bleeding profusely. About an hour had elapsed before my arrival, and the servants, becoming much alarmed by the bleeding, had kept pouring cold water in large quantities over the head and chest until the flow of blood ceased.

My first impression on seeing the state of the boy was, that the wound must have penetrated the chest, and that internal hemorrhage was going on; but finding, on examination with a probe, and afterwards with a round-pointed

director, that the wound was certainly exterior to the wall of the chest, and being also convinced, after percussion and auscultation, that no effusion had taken place into the cavities of the chest, I felt disposed to attribute this alarming state to the combined effects of loss of blood, fright, the overloaded state of the stomach, and, above all, the drenching with cold water, which had been persisted in for nearly an hour. I immediately gave him some ammonia, which was repeated at intervals, and afterwards warm tea and gruel; at the same time applying warmth in every possible way, and friction to the surface. After these means had been persevered in for two hours, his pulse improved; the natural heat began to return, and he felt more comfortable.

On visiting him next forenoon, I found that he had passed the night without much suffering, and at intervals had slept for a short time—his countenance was still rather pale, but not so anxious—his pulse 110, pretty full, but compressible and quite regular—his breathing somewhat oppressed, but not affected by change of position, although he seemed rather to prefer resting on his right side—the respiratory sounds on both sides were natural, and on percussion no dulness could be detected. He complained of some pain about the centre of the chest, which was aggravated by full inspiration; his breath still smelt strongly of sour fermented matter, and once or twice during the night he had vomited.

I opened a vein in the arm, from which blood flowed at first freely, but very soon caused faintness, with almost imperceptible pulse, so that only six or seven ounces were obtained. On the faintness going off, he expressed himself as much relieved, and shortly afterwards a purgative (which had been taken in the morning) acted freely, with still further relief. He had been several times out of bed during the morning, and had walked through the room.

In the evening I was informed that he was much worse, and on going out I found him in a most alarming state; he was delirious, extremely restless, and constantly tossing about in bed, calling on some one to remove the weight from his breast or he would die.

His breathing was quick and oppressed—his pulse weak, quick, and now, for the first time, irregular—his countenance alternately flushed and deadly pale, and expressive of great anxiety. An attempt was again made to bleed him, but only four or five ounces had flowed when deadly faintness was caused.

During the night these symptoms increased, and on Friday he gradually fell into a state of torpor approaching to coma, with irregular and very feeble pulse; but at times he could be roused, and endeavoured to speak, but with great difficulty in articulating.

He died on Friday evening, fifty-two hours after receiving the wound. Some hours before his death, an erythematous blush had come on around the wound, and also around the venesection wounds.

After his state had become so alarming, he was visited by my brother Mr W. Traill, and also by Dr Bruce of Arbroath, both of whom repeatedly examined the wound with a probe and with a blunt director, but without being able to ascertain that the chest was entered. We also repeatedly examined the chest by percussion and auscultation, and were satisfied that both lungs acted freely, and that there was no effusion into either pleural sac; but on the Friday it seemed to us that there was evidently effusion into the pericardium.

During the Thursday and Friday he was bled five times, always with some relief to the pain and feeling of weight over the chest. Antimony, so long as it could be borne, and afterwards ammonia, and twice a dose of calomel and morphia, were given.

On Saturday, fifteen hours after death, an examination of the body by order of the sheriff, was made by Dr Smith of Forfar and the medical men who had visited the young man previous to his death.

On *forcibly* passing the little finger into the wound, it at once touched the cartilage of the fourth rib, which was then found to be partially divided, and the finger could easily be passed into the cavity of the chest, and then came in contact with a sac evidently containing fluid.

On dissection, we found the wound to have commenced at the lower edge of the third rib, an inch and a half from the sternum, then to have passed obliquely downwards and inwards, entering the chest at the upper edge of the fourth rib, close to its sternal extremity; it then passed through the anterior mediastinum without opening the pleural sac, entering the pericardium on its anterior surface, and thence directly into the right ventricle of the heart.

The pericardium contained from eight to ten ounces of dark-coloured blood, partially coagulated, and constituted the sac, which was felt on passing the finger into the wound. There was no effusion into either pleural cavity. The heart was empty, and seemed of small size, pale coloured but otherwise healthy. Neither the heart, pleura, nor lungs presented any appearance of inflammatory action. On the inner surface of the ventricle, adhering to the orifice of the wound so as apparently to close it, there was a small clot of fibrine about the size of a pea, and of a regular spherical form.

The wound in the heart was situated about the lower third or middle of the ventricle, almost close to its junction with the wall of the left ventricle; on the external surface it was rather less than a fourth of an inch, and on the inner surface about an eighth of an inch in length; it divided the muscular fibres in nearly a transverse direction. The wound through the pericardium was also about a fourth of an inch in length.

The wound through the wall of the chest was much longer than that through the integuments, so that it easily admitted the finger through the opening in the intercostal muscles, without dilating that part which divided the cartilage of the rib. The cartilage was divided very obliquely, and evidently by a sharp cutting edge, so that the inner portion formed as it were a very thin slice.

The scissors with which the wound was inflicted were strong and coarse, with both blades alike and thick on their backs, but they were new and sharp-pointed.

It was some time before I could explain the existence of two wounds, one external to, and the other within the wall of the chest. But on considering the nature of the wound at its entry into the chest, its comparatively great length, and the clean incision into the cartilage, showing that at that point a sharp cutting edge had evidently come into action, and on comparing this with the scissors, which when closed formed an instrument, sharp-pointed no doubt, but with two blunt and thick sides; it then became clear, that on the point striking the sharp upper edge of the rib, a separation of the blades must have taken place—the one blade penetrating internally, and the other forming the wound over the sternum under the integuments.

ARTICLE IV.—*Cases of Rupture of the Urinary Bladder, with Remarks.* By EBEN WATSON, A.M., M.D.

(*Read before the Glasgow Medical Society, October 17, 1848.*)

CONSIDERING that rupture of the bladder is not an extremely rare occurrence, there are very few cases of it on record. The reason has been supposed to be an unwillingness on the part of surgeons to publish cases so frequently terminating in death, notwithstanding their best efforts to preserve the patients alive. The great fatality of this lesion ought, however, only to increase the interest

with which cases of it should be studied, while the opportunities thus afforded of obtaining, by inspection after death, exact information regarding the nature of the injury sustained during life, ought to be the more fully and anxiously improved.

Such cases are not, on the other hand, so frequent in their occurrence, that the experience of any one man can be sufficient to establish on a firm basis the pathology, prognosis, or treatment of this grave injury, either for his own guidance or that of other practitioners. Such a happy result can only accrue from a careful induction of numerous and well-observed cases. Nor do we know of any better source from which to derive the required data than are the records of the Glasgow Royal Infirmary, which, from its position in the midst of a populous mineral and manufacturing country, receives into its wards the subjects of more numerous and severe accidents than any other hospital in the kingdom.

The following cases occurred during my residence in the Infirmary as house-surgeon. They are given in nearly the same words in which they were originally inserted by myself in the hospital journals :—

CASE I.—William M'Culloch, aged thirty-two, a carter, was admitted into No. 11 Ward of the Glasgow Royal Infirmary, on the evening of August 13, 1844.

Three days before his admission, when the patient was in the act of leaping from a cart, which was the foremost of a line of several, his foot slipped, and he fell to the ground upon his back. While in this position, the wheels of three waggons passed between his legs, and over his left groin and thigh. His chief complaint, after the accident, was of pain in the perineum and above the pubis, with a constant desire but complete inability to pass his urine. The treatment consisted of bleeding from the arm, and by leeches to the parts affected. His bowels were freely moved by medicine.

On his admission, he still complained of pain in the perineum; his countenance was anxious, and his respiration rapid; pulse 113, feeble.

There was livid ecchymosis of the skin on the inside of the left thigh and groin, as also of the scrotum and penis. The cellular tissue of the upper part of the thigh, scrotum, and perineum, especially its left side, were much swollen, and communicated such a sensation as if the parts mentioned were distended with air and fluid. It was not at this time judged prudent to make such an examination as could enable us to detect fracture of the pelvis; but this being strongly suspected, perfect repose in the easiest position was enjoined, fomentations were assiduously applied to the injured parts, a Dover's powder was given, and wine was cautiously administered. The catheter was introduced with ease, and apparently into the bladder. A considerable quantity of bloody foetid urine flowed by it, and the patient felt much relieved.

Next day this state was much the same, but easier, on account of the frequent introduction of the catheter, by which a very little urine was on each occasion allowed to trickle away. A free incision was made by the late Dr Hannay, then surgeon to the hospital, on the left side of the raphe, and at the anterior part of the perineum. Thirty-two ounces of fluid, which had a strong urinous odour, were thus evacuated; and, when the finger was introduced into the wound, the ramus of the pubis could be felt fractured about its middle. The patient is reported to have experienced much relief on the evacuation of this urinous fluid. Pulse still continued quick and feeble, and his other

symptoms indicated much general prostration. It is also reported that the sensibility of the lower extremities remained unimpaired.

He was ordered 12 oz. of wine and 1 lb. of beef-tea in the day, and to have camphor and opium thrice in the twenty-four hours.

On the 15th the hospital report is as follows:—"Urine flows freely by the wound. Feels and looks relieved. Ends of bone (viz., of pubis, felt through the wound) at a distance from one another, and distinctly moveable. Tongue clean and moist. Pulse 90, improved in strength. Natural movement of bowels. Patient can raise lower part of body in bed. He relishes his wine, has some appetite, and does not vomit."

Next day he was ordered six ounces of whisky, instead of his wine; but there is no report until the 24th, when it is said that "patient has had several rigors, followed by heat of skin. Pulse rapid, small, and weak. Complaints of thirst and of weakness only. Bowels free; tongue moist, and slightly white. Urine flows freely by the wound, and a considerable extent of the ischium is exposed, by sloughing of the cellular tissue."

Dover's powder at night was substituted for the camphor and opium.

Next day (25th) it is reported that there is extensive sloughing of perineum and over sacrum; but that, in other respects, the patient is not worse.

27th. Debility and exhaustion increase. Respirations 40 per minute. Pulse scarcely to be felt, and tremulous.

He died next morning.

20th. *On inspection*, much effused blood was found beneath the pelvic fascia, and in the folds of the mesocolon and mesorectum. The bladder was found ruptured obliquely through the left side of its neck, and below the reflexion of the peritoneum, which constitutes its lateral false ligaments. The coats of the bladder were thickened and inflamed. The rupture was a clean rent through both coats, and admitted three fingers. It opened into what appeared a large abscess, stretching up behind the bladder and between it and the sacrum. The incision had been made into this cavity. The urethra was sound and uninjured throughout its entire length.

The left sacro-iliac synchondrosis was completely separated, and the left side of the body of the pubis was fractured close to the symphysis. The left ramus of the pubis and ischium was also fractured, and the fracture extended into the acetabulum. The tuberosity of the ischium was completely separated from the body of the bone. The cavity of the left acetabulum contained a quantity of purulent matter, and its lining membrane presented the usual appearances of high inflammation.

On the right side of the pelvis the only injury was an oblique fracture of the ramus of the pubis, without any displacement.

At the lower margin of the right lobe of the liver there was a small deposit of purulent matter. Other organs were quite healthy.

CASE II.—George Bell, aged twenty-seven, an engineer, was admitted into ward No. 11 of the Glasgow Royal Infirmary, on the 14th March 1846, at nine and a half P.M. (Dr A. Buchanan was at this time surgeon to the hospital.) Half an hour before his admission the patient was at work beside the steam-engine which he had charge of, when his left foot was caught by the fly-wheel, and he was drawn in among the machinery. The foot passed between two spokes, and the whole force of the revolving wheel came upon his leg, which was pressed against the belt, and the engine thus stopped. But before that was effected his right leg had been twisted upwards, and he had received a violent blow at the lower part of his back from the main shaft of the engine.

On examination it was found that the patient had suffered a severe compound fracture of both bones of the left leg; which was immediately attended to.

He complained much of pain at the right sacro-iliac synchondrosis, especially when the limb of that side was moved, and, with the exception of being able to draw up the leg a little, he had no power of moving it. The posterior part of

the right ilium was felt to project more than that of the other side, and it was more moveable on the sacrum, which seemed at a relatively greater depth from the surface of the back. In short, there appeared to be displacement of the sacrum forwards, and of the ilium backwards, on the right side.

Patient had an uneasy and ineffectual desire to micturate. Some bloody urine was drawn off by the catheter. His face and surface generally were pale and cold. Pulse quick and feeble.

He had been in perfect health up to the time of the accident.

Next day the patient was still very uneasy, and evidently feebler. He was unable to micturate by voluntary effort, but felt much relieved by the frequent introduction of the catheter; on which occasions small quantities of slightly bloody urine were usually evacuated.

The evident indications of supporting the failing strength, and allaying the sufferings of the patient, were attended to, but without avail, for next morning he was found sinking fast, and he expired at six P.M.

17th.—On inspection of the fractured leg, it was ascertained that the posterior tibial artery was sound, but that the peroneal was ruptured.

On examining the abdomen, the bladder was found ruptured at its fundus, so as to admit three or four fingers. The rupture was transverse, *i. e.*, nearly parallel to the transverse diameter of the pelvis, and its edges were clean. A very small quantity of urine was found in the peritoneum; of which that part which lines the pelvis was much injected. A large quantity of blood was effused into the interior of the pelvis, and into the substance of the muscles at the lower part of the abdomen, as well as between them and the peritoneum. The other abdominal organs were healthy.

The right ilium was quite separated from the sacrum at the synchondrosis, and the pubic ligaments were also ruptured, so that the os innominatum was thus completely detached from the rest of the pelvis.

The first point of interest in these cases is the mechanical cause of the rupture in the coats of the bladder.

Dr Harrison, in his excellent paper "On Rupture of the Bladder," contained in the 9th volume of the *Dublin Medical Journal*, (page 349), states two causes of this lesion, to which he refers all recorded cases of it. His words are, "every case on record has been the result, either of some force directly applied against the abdomen, such as a blow or fall against some resisting body, or of a fall from a height, causing a general concussion of the whole frame." We think it will not be difficult to show that the cases which have just been read, resulted from neither of these two causes; and it will be equally easy to point out a different, but a very obvious and sufficient, cause for the lesions sustained.

In the first of them the force was not applied directly against the abdomen, but across the groin; neither does it seem that the bladder was ruptured by the general concussion, which, no doubt, was caused by the fall from the cart; for in all recorded cases of this last-mentioned kind, *viz.*, those in which rupture has resulted from "a general concussion of the whole frame," the seat of the rupture is at the base of the bladder. This happens because, first, the base is the least distensible part of the organ, it being covered with peritoneum: and second, because the urine, which in these cases is the efficient cause of the rupture, is driven, by the impulse communicated to it, against the neck of the bladder and pubic bones, is by them resist-

ed and made to recoil against the base, where it operates, if the force be sufficient, by rupturing first the peritoneal, and then the other coats of the viscus. But the situation of the rupture in the case under consideration, was very different. It was in the neck of the bladder, and at that part of it, too, which exactly corresponded in position, extent, and direction, to the loosened part of the pubic bone. It ought to be remembered that the body of the pubis was completely fractured a little to the left of the symphysis, and that the corresponding ramus of the pubis and ischium was also fractured. This piece of bone, then, which corresponded so exactly with the rent in the bladder, was completely severed from its bony connexions, and might easily be, as we think it was, driven in upon the distended bladder. From a consideration, moreover, of the manner in which the external force was applied, and from the directions of the other fractures of the pelvis, formerly detailed, it will be readily perceived that a continuation, or, as really happened to this poor man, a repetition of the original force was quite capable, nay, most likely, to bear this fractured piece of bone against the part in which the rupture of the bladder was found after death. In explanation of the important bearing of these observations on the case in point, we need only refer to the history of it previously given.

In the second case, that of George Bell, the force which caused the rupture acted from behind. It is somewhat difficult, however, to determine precisely how the blow operated, whether by producing a general concussion of the whole frame, or by causing the bones to impinge directly on the base of the bladder. We think, however, that the nature of the accident and its obvious effects, sufficiently prove the latter supposition to be correct. It will be remembered that the hour at which the accident happened was late in the evening, when the man was about to quit his work for the day, and very probably he had allowed his urine to accumulate in the bladder for some time previously. It is not too much then, we think, to assume that the bladder was fully distended, when the man received the blow from the main shaft of the engine. This blow immediately detached the ilium from the sacrum on the right side, and ruptured the pubic ligaments, thus separating the os innominatum from the rest of the pelvis. Nor was the force of the blow exhausted in producing this separation; for the right margin of the sacrum was at the same time pressed forwards—a position which, owing to the nature of its other connexions, it retained even after death; thus indicating, with perfect certainty, the minimum extent of the indentation. Even apart, therefore, from a consideration of the violence of the blow, and the natural elasticity of the parts, such a change in the bony pelvis as was found after death, was amply sufficient to prove that the sacrum and the margin of the ilium had been brought into direct and violent contact with the base of the distended bladder. There is, however,

reason to think, that had this blow been the immediately efficient cause of the rupture, the rent in the bladder would have corresponded with the direction of the synchondrosis, and would not have been transverse. There seems to have been another link then in the sequence of events; and the history of the accident probably was, that the blow of the main shaft drove in the bones at the right synchondrosis against the base of the distended bladder, and that thus the violent impulse was communicated to the urine, which, by its rebound, produced the lesion in question.

In both of these cases, the accident was of a very severe kind, and the nervous shock proportionately great; but the progress of the cases was by no means such as it would have been had the bladder remained entire. The prominent symptoms in both cases were those occasioned by the lesions of the bladder; and yet their character and course were materially different.

As regards the case of the man M'Culloch, we, unfortunately, cannot speak with precision of the state into which he was immediately thrown by the accident, seeing that he was not admitted into the hospital until three days thereafter. But, judging from the severe antiphlogistic treatment which he appears to have required, and to have borne, his state must have been one much more acutely inflammatory than that of George Bell. The latter, indeed, never recovered from the nervous prostration caused by the effusion of urine into the peritoneum, combined with the shock of the accident. The latter, although severe, would not, we think, have been adequate to the effect produced, had not the former concurred with it. The patient in this case lived forty-five hours, and expired without exhibiting the slightest symptom of reaction. Such a termination of the case will not astonish any one, however, who remembers the effect of a foreign fluid in the peritoneum, especially when of an irritating nature, and suddenly introduced, as was the urine in this instance.

M'Culloch, the subject of the other case, lived for eighteen days after the accident. The "shock" which he received at first, was not indeed so severe as that under which the other man sunk, but the injury to the pelvis was much greater. The true explanation of the great difference in this respect between these two cases, is to be found in the fact, that in the latter the urine was not, as in the former, discharged into the peritoneum, but into the muscles beneath it. Inflammation, suppuration, and sloughing ensued; the tendency being towards a natural cure by external opening of the abscess. This effort of nature was duly aided by surgical interference; and there is little doubt in my mind, that this patient would have ultimately recovered, had he not fallen a victim to that mysterious disease phlebitis, which was then prevalent in the hospital. The commencement of this disease in M'Culloch's case, is noted in the report of the 24th, up to which time he had gone on improving. Consequently, at the inspection, it was found that one of those

peculiar purulent deposits, characteristic of that disease, existed in the right lobe of the liver, and that a quantity of matter was contained in the left acetabulum. This case, then, is in many respects similar to that of the child Nicholas Keog, recorded by Dr Harrison in the paper formerly referred to. Regarding this case, Dr H. remarks, that he thinks it might have been saved, had he sufficiently early used free incisions, so as to admit of the escape of the urine, and thus to check the diffuse gangrenous inflammation of which the child ultimately died. In his *Pathology and Practice of Surgery*, Mr Syme has lately recorded an interesting case of rupture of the neck of the bladder, from direct violence applied above the pubis, and without fracture of the pelvis. The skin was freely incised in the place indicated, the patient's strength was supported, and after a critical illness he recovered in about six weeks. This, we believe, is one of the only two cases on record, of recovery from rupture of the bladder. The true principle of prognosis in these cases, has been laid down by the same high authority, that "if the rupture takes place above or within the reflexion of the peritoneum, there cannot be the slightest chance of escape. But if the rent is at the anterior part, so as to discharge the contents of the bladder by a sudden gush into the cellular substance, and condense it in such a way that only the portion in contact with the urine may be deprived of life, it appears that the patient may be saved by timely incisions." This latter observation is equally applicable to such cases as that of M'Culloch, formerly related;—the only difference being in the position of the rupture, viz.—in the side rather than in the front of the neck of the bladder. Mr Syme, in penning the remark, obviously referred to cases similar to his own, in which no fracture of the pelvis existed; and, in these circumstances, the only part of the bladder which could suffer rupture without involving the peritoneum, is the anterior part of its neck. But in cases of fracture of the pelvis, a large portion of the sides, and of the inferior part of the bladder, is liable to rupture, while the peritoneum may not be injured.

In regard to treatment as well as to prognosis, cases of rupture of the bladder are divisible into two classes, viz., those within and those without the peritoneal sac. The great feature of treatment in the former class of cases is, incision for the relief of the extravasation, which is the immediate consequence of such ruptures. In all cases of this kind there will be some external mark of the phenomenon in question—such mark will be afforded by swelling, as in Mr Syme's case, or of that combined with crackling, as in the first of the two cases related in this paper. It is extremely unlikely that any such mark should be present if the bladder were ruptured within the peritoneum; for in that case extravasation would be prevented by the peritoneum; and, owing to the nature of the causes which generally operate in the production of such injuries, no apparent phenomenon may indicate rupture of the bladder. A

blow on the vesical region of the abdomen may be sufficient to produce this lesion, and yet no mark of violence may be visible on the skin; a circumstance which is remarkable in all abdominal injuries. In general concussions of the whole frame, on the other hand, the external marks of injury may be at a considerable distance from the seat of the most important lesion. It is fortunate, however, that in such cases, viz., those in which there is no external mark to direct the surgeon in making his incisions, the situation of the rupture being most likely within the peritoneum, there is no hope of relief from such practice.

The question now naturally occurs, What is the treatment proper for intra-peritoneal rupture of the bladder? or, Is there none? Dr Harrison recommends tapping of the peritoneum from the rectum; but the operation has not been tried. Certainly, if such an operation is to be at all successful, that must be the situation of the opening into the peritoneum. There could be no expectation of benefit from paracentesis in the usual place, where it was performed in two fatal cases of this kind, mentioned by Dr Harrison. This practice seems to me well worth trying in all such cases, in which the bladder is reported to have been much distended at the time of the accident. It can add nothing to the danger of the patient, and seems to give him the only chance of life. This opinion is strongly corroborated by an interesting provision of nature which has been found to occur in some of these cases, such as Gibson's case, related by Dr Harrison, and another mentioned by Dupuytren in the *Archives Générales* for June 1834. In both of these cases the lymph thrown out by the peritoneum had formed a pouch or secondary reservoir for the urine, by agglutinating the intestines and the fundus of the bladder itself to the walls of the abdomen, thus shutting out the pelvic cavity from the rest of the abdomen. Now, in these cases, if there had been some outlet for the urine, the inflammation would, in all probability, have subsided, and the men would have recovered. In other instances, however, a small quantity of urine having been at first effused, it seems to be rapidly absorbed in the early stage of the inflammation which it occasions. In these cases, then, no good could be done by operative interference of the kind alluded to. The *secretion* of the urine seems in them to be all but suspended, and therefore the passage of that fluid from its natural reservoir would not be sufficient to call for such procedure.

The difficulty of diagnosing between these two sets of cases, is often extreme: but perhaps the absence of every symptom except those of rapid failure of the vital energies, contrasts sufficiently with the phenomena, more or less apparent, of acute peritonitis, to enable the observer to distinguish, in most instances, those in which there is much from those in which there is little urine effused into the peritoneum. In the latter class of cases, the ordinary constitutional treatment of acute peritonitis—venesection, if the patient is seen

in time, the free administration of calomel, opiates, and warm water enemata, with fomentations to the abdomen and complete rest—must be relied upon alone: nor ought such means to be neglected in the former class of cases, whatever else may be done for the patient. It is but too likely, from the nature of these cases, that if the patient live long enough stimulants may be proper; but they should be given with great care, and watchfulness as to their effects. In all such cases the anxiety of the surgeon will naturally be directed to the prevention of the accumulation of the urine in the bladder, which, if it did occur, even in a small measure, might endanger a new effusion of urine into the abdomen. This indication will be fulfilled, so far as possible, by the frequent introduction of the catheter, or, if the patient bear it well, by leaving a gum catheter in the urethra, care being taken not to push it beyond the neck of the bladder, lest, by adding a new cause of irritation, the existing inflammation be increased or renewed.

ARTICLE V.—*Contributions to Toxicology. Cases of Poisoning, with Remarks.* By DOUGLAS MACLAGAN, M.D., F.R.S.E.—
(Continued from p. 295.)

Acetate of Lead; Attempted Suicide; Imputed Poisoning.

ELLIOT MILLAR, cloth-finisher, Galashiels, and his wife, had been married for about thirteen years, but for two or three years had lived very unhappily together. One of the parties had communicated syphilis to the other, and, considering the otherwise indifferent character of the husband, there seems to be little room for doubt that he was the culprit in this matter.

Millar's bad usage of his wife had on several occasions induced her to leave his house, and seek an asylum under the roofs of her relatives, and had led her about two years previously to attempt to poison herself with sugar of lead. On Saturday, 3d July 1847, she again resolved to destroy herself, and for this purpose sent one of her sons to a laboratory to purchase a pennyworth of sugar of lead, the whole of which she dissolved in water, and swallowed before going to bed. The boy was instructed to say that the sugar of lead was to be applied to a sore; the quantity furnished to him was half an ounce; it was wrapped in paper, and labelled "Sugar of lead. Poison." This paper she put into her pocket, after swallowing the drug. During the night and following morning, she was sick, and vomited, but suffered no other inconvenience. On rising next morning, Sunday 4th July, she threw out the vomitings, and, as she slept in a separate apartment from her husband, he was not aware of what she had done.

She then prepared coffee for her own and her husband's breakfast. He drank two or three cupfuls of the coffee, she herself

drank two cupfuls at the same time, and another after he had finished his meal. This interruption to her breakfast, arose from his having reproached her with being more active at taking her meat, than in attending to her household duties. This was the beginning of a fresh altercation, in the course of which she remarked, that had she been that morning where she intended, she would not have been there to bear his abusive language, and thereupon she showed him the paper marked "sugar of lead, poison," and told him of her suicidal attempt. He made no further remark at the time than that he did not believe she had attempted suicide, and he left the house. He went up to the house of his brother, who lived above him, and told him of what his wife said she had done, and then said to him that he felt unwell, and believed that he had himself got the poison. His brother advised him, if this were the case, to go to the laboratory and procure a vomit. He accordingly went to the chemist's, showed the labelled paper, again expressed his belief that he had got the poison, and was accordingly furnished with a scruple of ipecacuan, and a grain and half of tartar emetic, with directions to return in half an hour if it did not operate. He went home, mixed the emetic in water in presence of his wife, and carried it into an adjoining room, where he said he swallowed it. He then went up again to his brother's house. The emetic said to have been taken by him, not having operated, nor produced even an appearance of nausea, he was advised by his brother and father to go back to the laboratory again, which he accordingly did, but not for more than an hour after the time when he got the first emetic. The druggist's assistant on this occasion asked him if he "felt a pain in his bowels, and a sweet metallic taste, and if he felt sick," when he said he felt the pain in his bowels, and a slight taste, but not sick. At this moment a medical man, Dr Hutton, happening to pass, was asked to give him the benefit of his advice. Millar, according to the druggist's statement, did not seem apprehensive of himself, nor did he appear to be suffering; when, however, Dr Hutton came in, he seemed "a little afraid." The doctor inquired as to his symptoms; but he said now he had none; "no headache, no pain, no spasms, no sickness; indeed, no symptom whatever." The doctor seems very properly to have attached no importance to the case; he bade the apothecary repeat the emetic, and left him.

Millar carried home his second emetic, and along with it some vinegar, part of which he drank himself, and he made his wife drink some also. This extraordinary antidote for acetate of lead seems to have been a prescription of his own. This second emetic was undoubtedly swallowed, and it operated, speedily producing so much nausea as to oblige him to lie down on his bed. He vomited into a basin, and this, with its contents, he placed, apparently for concealment, in a tub under a bed in a closet, where they were seen

by his wife next day, and left untouched by her. At the time when Miller first accused his wife of having poisoned him, and when the breakfast things were still standing on the table, he took the coffee-pot, and poured out a little of the coffee which remained into a cup. This he placed in a drawer, which he locked, and put the key in his pocket. To all his accusations, his wife protested strongly that she was entirely innocent of poisoning him, as she had no poison in her possession, except that which she had swallowed the previous night. Next day, Monday, July 5, a constable, who had been informed of Mrs Millar having attempted to poison her husband, came to the house, found Millar and his wife there, and got from the former the portion of coffee which he had locked up. The constable returned again in the evening, and then got the vomitings. Both of these he put into bottles, and, having secured them, marked them for identification. On the succeeding day, Tuesday, the procurator-fiscal came, and he, having made some inquiries on the subject, a warrant was obtained, and Mrs Millar committed to prison on the charge of having attempted to poison her husband.

The usual inquiries were now instituted by the authorities, with a view to bringing her to trial for her alleged crime. The prisoner, as usual in such cases, was herself examined on Tuesday the 6th July, and her declaration taken down, and afterwards signed by her. It was again read over to her on the succeeding day, some additional particulars added, and adhered to by her. It contained merely a statement of the facts mentioned in the above narrative, so far as known to her, and a strong protestation of her innocence. Her apparently candid and straightforward manner under examination, had impressed the procurator-fiscal and justice of peace who examined her, very favourably towards her; so much so, that the former, in reporting the case to another of the authorities, actually states his suspicion, that the sugar of lead, which by this time had been tested for, and found in the vomitings and coffee, may have been put into them after they were set aside.

Elliot Millar himself was on the same day, 6th July, examined in precognition as the principal witness against his wife. This evidence was a repetition of his statement as to her having given him the poison in his coffee, admitting, however, the fact, so favourable to the accused, of her having at the same time herself partaken of the coffee. He ascribed their domestic quarrels to her inattention to her domestic duties, and especially to her having contracted and communicated to him venereal disease. He admitted having struck her; but his whole evidence tended to gloss over his brutality towards her, and to magnify all the faults which he imputed to her. He assigned to the magistrates at this examination, as the ground of his belief of the present crime, her having tried to poison him with sugar of lead about two years previously. This statement, which pointed to the former occasion when she

had attempted to destroy herself, was clearly an untruth. At that time he accompanied her to a medical man, Dr Weir, for the purpose of procuring antidotes to the poison which she had taken; but he never breathed a syllable as to her having given any poison to him, and he sought no advice for himself, but only for his wife. Moreover, having occasion to see Dr Weir soon after the present precognition, when his wife was in jail, he gave to the doctor a quite different reason for believing her guilty. He made no allusion to any attempt with sugar of lead two years previously, because he knew Dr Weir to be cognizant of the facts as they then occurred; but he said that she had tried to give him prussiate of potash in his porridge some months before, his acquaintance with this salt arising from his knowledge of its employment as a dye-stuff. Dr Weir at once set down this story as a falsehood; but the inconsistency of his statements to the doctor in private, and to the magistrates at their examination, was not known at the time. Nevertheless, the general character of the statements made by Millar at his precognition, inspired the procurator-fiscal who examined him, with a want of confidence in their truthfulness. Among other suspicious circumstances eliminated, was the fact, that he had himself been in possession of sugar of lead as an application to his venereal sores shortly before this time; but he alleged, that, as he had found it useless to him, he had put it into the fire. The procurator-fiscal, struck with the peculiarity of the whole case, did not think it right to proceed further without consulting the legal advisers of the crown; and therefore he transmitted the various documents to crown-counsel for their opinion. In consequence of instructions from them, he again on 15th July examined Elliot Millar, who adhered to his former declaration of the 6th, and made one unimportant statement in addition thereto.

This being reported to the authorities in Edinburgh, instructions were sent to the procurator-fiscal, to subject Millar to a further examination still, which took place on 31st July. His former declarations were read over to him; but he declared at the outset, obviously with a view to cutting short his examination, that this was unnecessary, as he perfectly remembered all that he had said. To all the questions put to him he answered promptly and decidedly, and he showed very manifestly throughout, the desire to make out a case as strong as possible against his imprisoned wife. At length, towards the close of the examination, the procurator-fiscal asked him directly, if he did not himself put sugar of lead into the coffee on the Sunday morning. He said he did not. The question was pressed a second time, when, after some shuffling and equivocation about his telling nothing but truth, he at length confessed that, having still in his possession a portion of the sugar of lead which he had been using for his venereal sores, and which he had declared he had burned, he had, during his wife's temporary absence from the house, put a pinch of it into the coffee which he had

locked up in the drawer, and thrown the rest among his own vomitings.

This confession speedily made an important change in the relative positions of the parties. Mrs Millar was liberated, and Millar himself was committed to prison, eventually indicted under the wholesome but comprehensive Scottish common law, for "falsehood, fraud, and wilful imposition," and specially, for "wilfully, wickedly, and feloniously, accusing an innocent person to the public prosecutor, as being guilty of a heinous crime, for the purpose of preventing the administration of public justice." He was found guilty on both counts of the indictment, and sentenced to transportation. At the trial, before the Circuit Court of Justiciary, the testimony of the procurator-fiscal, and the rest of the general evidence, was so conclusive, that no important medical or chemical evidence was called for; and I was examined merely to identify my reports, and to prove the fact that there was sugar of lead in the coffee and vomitings.

Millar made an ingenious attempt to get out of the dilemma in which he found himself placed, by the confession of his fraudulent conduct being extorted from him. He forthwith averred that he had said this to the procurator-fiscal merely to get his wife out of prison, and he actually took credit to himself for having procured her liberation. But, unhappily for this pretended conjugal devotion, there was an awkward letter of his in existence, in which he threatened her, if she did not agree to certain requirements of his regarding his children, with being "immediately apprehended for leaving the house when he was at his work, and robbing it of different things which she had no right to carry away." He seems to have had a determined intention to get rid of her by *legal* means, in one sense of the word.

To preserve the continuity of the narrative, I have said nothing hitherto as to the chemical evidence in this case. The coffee and vomitings had been chemically examined on the 5th July, the day after the alleged poisoning, by Dr Macdougall of Galashiels, who reported that both were strongly impregnated with sugar of lead; and these articles were afterwards transmitted to me for corroborative analysis. A few drops of the liquid part of each sufficed for determining the presence of lead by the ordinary tests of sulphuretted hydrogen, chromate of potash, and iodide of potassium. Although, however, the presence of lead was all that I was called to report on at this time, I did not feel contented with determining its existence in the articles sent to me, but thought it right to ascertain its amount also. I was induced to do so from the consideration, that, as sugar of lead is a substance very frequently kept in families for purposes of domestic surgery, it would be a very likely defence set up by a person accused of using it as a poison, that some such thing as a lotion, or other preparation of it, had been taken or given by inadvertence; and I thought that a com-

parison of the quantity found in the coffee and vomitings with that in any lotion, supposing the strength of the latter to be approximately known, might furnish some evidence of the guilt or innocence of the accused.

The method which I followed for determining the quantity of lead present was the following:—The vomitings constituted a turbid white mixture, of sour smell, which, by rest, deposited a quantity of white flocculent matter, like coagulated milk, and some pieces of flesh, apparently salted meat, leaving a clear pale yellow liquid above, on which some fat floated. It was in a small portion of this clear liquid, that the tests formerly mentioned, so distinctly showed the presence of lead. The deposit from the vomitings obviously contained much of the metallic salt also, as it deeply blackened under the action of sulphuretted hydrogen. The whole was digested with a considerable proportion of pure nitric acid, and filtered; and to the filtered acid liquor, pure sulphuric acid was added. The sulphate of lead thus thrown down was washed first by subsidence, and afterwards on a filter. The solid matters separated by filtration, were collected and incinerated in a crucible, the ash treated with nitric acid, the liquor filtered, and precipitated by sulphuric acid. The precipitate was united on the filter with that obtained from the liquid portion. It was further washed and ignited, to destroy some adherent animal matter. As a portion of the sulphate was thus reduced to the state of sulphuret, it was carefully heated in contact with some nitric and sulphuric acid, and weighed. The quantity of sulphate of lead thus obtained was 86·77 grains, which corresponded with 109·09 grains of common crystallized acetate of lead—($\text{Pb O } \bar{\text{A}} + 3 \text{ HO}$). A similar process was followed with the coffee, which had by rest deposited a heavy brown sediment, leaving a nearly colourless fluid above. The quantity of sulphate of lead obtained from the fluid ounce and a half of coffee sent to me, was 8·22 grains, corresponding with 10·3 grains of sugar of lead.

These results were duly reported to the authorities, who, in consequence of my directing attention to the quantity of the poison, submitted to me some further points for inquiry; the chief of these were the following:—

1. The cup from which Millar took his coffee being sent to me, to determine, what amount of sugar of lead would be contained in three cupfuls of the coffee (the quantity stated to have been swallowed by Millar), assuming it all to be impregnated in the same degree with the coffee analysed.

2. Keeping in view the fact, that the coffee sent to me was the last of five or six cupfuls which were in the coffee-pot, to state whether the cups first drawn off were likely to be more or less impregnated with sugar of lead, supposing a quantity of that salt to have been put into the pot while the coffee was making.

3. Having ascertained the quantity of sugar of lead which three

cupfuls would yield, to say, whether the vomited matters show more than could have been derived, or probably were derived, from such a source as the swallowing of the three cups of coffee.

4. On the assumption that any sugar of lead was added to the vomited matters after they were discharged from the stomach, to say, whether any test exists for distinguishing the result thus produced, from the result which would have been produced if the same sugar of lead had been infused in coffee first, and then swallowed and thrown up.

Some other questions were put along with these, but they were unimportant.

In relation to the first of the queries, I found, on measuring the cup sent to me, that when filled up nearly to the brim it contained $6\frac{1}{2}$ fluid ounces; that it could be made to hold $7\frac{1}{2}$ without overflowing; that I should estimate the average quantity which such cups would hold, when liberally filled, at 6 fluid ounces;¹ that supposing 10·3 grains of acetate of lead in a fluid ounce and half, to represent the proportion of that salt contained in the coffee generally, three cupfuls of six ounces each would give 123·6 grains, or about 2 drachms, as the quantity swallowed in the coffee. To this, I added, as reply to the third question, that the quantity contained in the vomitings was rather less than this, being 109 grains, and that the loss might be accounted for by some of the fluid having been used in testing for the presence of lead, as well as by some being absorbed from the stomach, and perhaps some lost in the act of vomiting. To the second query it was replied, that there would probably be rather more lead found in the coffee which remained at the bottom of the pot, than in the portions first poured off, on account of the partial subsidence of the insoluble compound formed by the lead with the vegetable matter of the coffee. That this, however, could not be to any great extent, because, if the coffee had been allowed to settle down thoroughly in the pot, its altered appearance must have attracted attention. It was, therefore, most likely, that the insoluble matter was kept suspended pretty equally through the coffee by agitation, and would remain suspended for some little time. Still, it was probable that the portion of coffee at the bottom of the pot would contain rather more lead than that first poured off.

The fourth query admitted of no reply but this; that I knew of no chemical means by which it could be determined that the sugar of lead had been added to the vomitings after ejection from the stomach. The fact, that the quantity found in the vomitings was correspondent with, and rather less than, the proportion contained

* This estimate was founded on the supposition, that the cups would not be uniformly filled to overflowing, and that allowance must be made for the addition of milk, &c.

in the coffee, was favourable to the supposition that it had really been taken in this form.

I point out these details, because they afford an excellent illustration of the way in which one kind of evidence, if judged of by itself, might lead to erroneous conclusions. The correspondence between the quantity found in the vomitings, and the proportion which probably would have been swallowed in three cupfuls of coffee, such as that analysed by me, is a remarkable coincidence. Yet we know, by Millar's confession, that he did it roughly, by putting a pinch of the sugar of lead into the coffee, and then throwing the rest among the vomitings. Had the fluid ounce and half of coffee contained a few grains less, the disproportion between that and the amount found in the vomitings, would at once have convicted him of false dealing; but it accidentally happened that the proportions were such as to be fairly adducible in support of the truth of his statement. There can be little doubt, that had Millar's acknowledgment not been drawn from him by the acuteness of the procurator-fiscal, and had his wife been brought to trial for her imputed crime, with such an unscrupulous witness to testify against her, and his statements backed by the probabilities of the above chemical evidence, she could hardly have escaped condemnation for an offence of which she was totally innocent. But, to make the proof complete against her, it must have been necessary to bring out some additional facts, which could not have been easily substantiated. The statement made in the chemical report as to the effect of subsidence in altering the appearance of coffee containing sugar of lead, must have compelled the public prosecutor to prove that the coffee, at the time it was poured out for breakfast, must have been kept agitated in such a way, as to keep the insoluble matter formed in it, in a state of suspension. Every chemist knows that acetate of lead is the very agent employed to decompose and decolorize coffee, in preparing its characteristic constituent, caffeine. The precipitate which the lead salt forms in its infusion, if it is allowed to rest, subsides, and leaves a pale-coloured fluid, in no respect resembling that which people are accustomed to drink as coffee. To this fact the attention of crown counsel was specially directed; and a very simple experiment showed, that if Millar took his breakfast with ordinary deliberation, the altered appearance of the coffee must have attracted his attention.

One ounce of ordinary coffee, in bulk nearly half a tea cupful, was boiled for ten minutes in the coffee-pot used by Mrs Millar, with six cupfuls of water, the total quantity prepared for breakfast by her on July 3d. It was allowed to settle for five minutes, and two fluid ounces poured off into a bottle. The decanted portion was of the ordinary appearance of unclarified coffee, dark brown, slightly turbid, and depositing some coffee grounds.

Half an ounce of sugar of lead, being the same proportion to this bulk of fluid as was found in the coffee got from Millar, was

now added; the coffee was boiled again, and allowed to settle for five minutes after removal from the fire. Another similar portion, being decanted, was now found to be a clear transparent liquid, with hardly any colour, except a faint shade of green, and more resembling a weak infusion of green tea than coffee.

It was obvious, therefore, that if during the breakfast the coffee pot remained at any time at rest for five minutes, the next cupful poured off must have been so different in appearance from ordinary coffee, as at once to have attracted attention. The bottles, with the two samples of fluid prepared as above, were placed in the hands of the crown-officers, and inserted, as is required by the Scottish criminal law, in the list of productions to be used against Millar; but from the strength of the case against him in other respects, crown counsel dispensed with this evidence, which would have gone to prove, that it was, if not impossible, at least very improbable, that the acetate of lead could have been administered to him in the way alleged.

The taste of the sugar of lead, must also have attracted the notice, of any person who got such coffee, as that analysed by me. It is true, that, in reply to the druggist's direct question, Millar stated that he had felt a sweet taste. But coffee so impregnated with sugar of lead, has such a marked sweet astringency, that no one could have swallowed a spoonful of it, much less three cupfuls, without being stopped in so disagreeable a process of deglutition. Still less is it likely, that this would have passed unobserved, by a person who averred, that the very party now suspected, had tried on a former occasion to destroy him, with the very poison now alleged.

This case does not afford otherwise much subject for remark, but it seems worthy of being recorded, as a most atrocious and deliberate attempt to impute crime to an innocent party, deservedly punished.

In addition to the general, moral, and confessional evidence, by which Millar's fraud was brought home to him, I would specially indicate as worthy of attention, one feature in the case which appears to be of some importance, as characterising an individual falsely alleging himself to have been poisoned. I allude to his indifference and want of anxiety as to procuring medical aid for his own relief. In the case of Leith, who pretended to have got arsenic from his wife, [November No. p. 284,] it was observed, that instead of being anxious for medical aid, he seemed indifferent to it, and was not so alarmed as his visitor who found him sick in his shop. The same indifference characterised Elliot Millar. He went to his brother, and told him he believed he had got the poison, but he never seemed to think of getting any medical advice, till his relative suggested to him to go to the druggist's and get a vomit. That much he certainly did, but it is more than doubtful if he swallowed the dose. He ostentatiously mixed it in his wife's pre-

sence, but without any assignable reason, he carried it into another room, where he said he swallowed it, but it did not produce even the least nausea. It must be allowed, that it is possible that a scruple of ipecacuan, and a grain and half of tartar emetic, may have been really swallowed, and have produced no effect; but it is at least a suspicious circumstance, that when the dose was repeated about two hours afterwards, and undoubtedly swallowed, it not only produced full vomiting, but caused so much nausea as to make him lie down in bed. To this it may be replied, that this is no more than is often observed with fractional doses of tartar emetic repeated at two hourly intervals, where nausea and vomiting frequently follow a second or third dose, where the first has produced no such effect. Admitting, however, that he did swallow the first emetic, the very failure in the means to get rid of his poison, especially as he now declared to his father and brother that he felt much worse, ought to have increased his alarm. But nothing of the kind was remarked. He had been desired to go back to the druggist's in half an hour if his first emetic failed, but, instead of this, he allowed nearly an hour and a half more to elapse, and he did not go back at all till his friends urged him to do so. Such præternatural coolness contrasts strongly with the flurry and solicitude about medical aid, generally manifested by persons, who believe themselves to have swallowed any thing deleterious.

If we believe Mrs Millar's account of her own conduct, we have another, added to many recorded instances, of the comparative inactivity of acetate of lead as a poison. Half an ounce was the quantity she got, and she affirmed on oath that she swallowed the whole of it. It is true that no one saw her swallow it, nor did any one witness any serious symptoms produced by it; for she states that she took it at night, in a room where she slept in a bed by herself, and its effects had almost entirely gone off by next morning. Nevertheless, she was seen by her son to vomit in the morning before breakfast, without any other obvious cause; and, although she might have a motive for pretending to her husband to have attempted suicide, her established truthfulness in every other particular, entitles her to be believed in making this confession also.—*(To be continued.)*

ARTICLE VI.—*Some Account of the Practice in the Cholera Hospital in Surgeon Square.* By WILLIAM ROBERTSON, M.D., F.R.C.P.
Ed. Physician to the Edinburgh Royal Infirmary and Cholera Hospital.

THE Cholera Hospital has been opened for little more than a fortnight, and although in this short time it has received 131 patients, it is of course impossible as yet to give a fair statement of the

deaths and cures, *i. e.*, a statement of the slightest statistical value. Of 131 admitted, 76 have died, 28 have been dismissed cured, and 27 (for the most part convalescents) remain under treatment. The proportion of female patients has exceeded that of males in the ratio of 88 to 43. Of 88 females admitted, 10 have been pregnant, 8 have been nursing, and 8 have been suffering from obstructed menstruation or other uterine complaints. Of those ascertained to be pregnant, 9 died, 4 having previously aborted; one, who likewise aborted, seems now well advanced in convalescence. It is remarkable, that in several of the cases occurring in nursing-women, the mammary secretion was not arrested by the attack of cholera. In three, the breasts ultimately became so much distended as to cause great inconvenience to the patients, and to require the use of the breast-pump at short intervals. Dr Douglas Maclagan informs me, that the milk of these patients differs very materially in composition from healthy milk,—that it is of low specific gravity, contains little or no butter, and, when examined microscopically, presents an unusually small number of milk granules. In several of the post-mortem examinations made at the Infirmary, appearances have been observed in the uterus and its appendages indicative of recent menstruation, and some of my patients have distinctly admitted that they were seized with cholera during a catamenial period. It seems worthy of further inquiry, whether there is any thing in the states of pregnancy, lactation, or menstruation, which renders females peculiarly susceptible to the influence of the choleraic poison.

The *poor* have, with a very few exceptions, been hitherto the sole victims of the disease in Edinburgh, and their liability to its attacks seems to be directly proportionate to their wretchedness. The dissipated have, as usual, suffered in greater proportion than the temperate. Cases occurring in habitual drunkards have been distinguished by great malignancy, and in several instances have immediately supervened upon the abuse of spirits. Poverty, filth, insufficient or improper food, residence in an overcrowded locality, or in one in which other cases have been observed, seem the most frequent predisposing causes. The question of contagion I shall for the present avoid, but think it right to state, that three nurses of the Cholera Hospital have already died of the disease. One of these women was a confirmed drunkard;—of the other two, one was of delicate constitution, but, I am assured, of sober habits; the other, to all appearance, a strong, healthy, and respectable person. In none of these three cases could I discover that there had been any communication with the sick, except in the hospital. The building in Surgeon Square is extremely well ventilated, regularly fumigated with chlorine, and in all respects very different from those miserable dwellings which cholera commonly visits.

Space will not permit me to give an account of the symptoms presented by my hospital patients; and as the object of this com-

munication is not to describe the *disease*, but the practice adopted for its mitigation, I shall pass at once to the head of treatment, under which certain symptoms will be incidentally considered.

Bloodletting has been tried in seven cases, in all of which the patient was seen early in the disease. In all these cases, there was vomiting and purging of the characteristic nature; in several, cramps were complained of; in all, there was a tolerably full pulse; in six, all of which are now either cured or convalescent, the temperature had not fallen much, if at all, below the natural standard; in one, blueness of the extremities had commenced; this last case was fatal. I have not practised venesection upon children, upon aged persons, or upon those who seemed of feeble constitution. The quantity of blood drawn has varied from twelve to twenty ounces; the operation has usually promptly relieved the patient from the cramps; and opiates given immediately afterwards have acted well. Collapse has either not succeeded, or has been very slight. I think that venesection, if practised early and upon suitable subjects, will be found to be at least *good* practice, probably the best that can be adopted. If, however, it be deferred till the algide symptoms have established themselves, the experience of most British practitioners condemns it as mischievous, and I am not disposed to question the accuracy of their conclusion.

The means of applying heat, to which recourse is commonly had in the cold stage of cholera, are tinned-iron vessels filled with boiling water, wrapped up in blankets and plentifully distributed round the patient's body in bed, frictions with turpentine, &c. These remedies seem always agreeable to the patient, and procure some mitigation of the cramps, but are seldom observed materially to restore the heat of the surface. A more powerful agent is the hot spirit-bath, which was often used in the Infirmary among our earlier cases, and, I believe, with very little good effect. A plan which I have adopted of late has been found more efficacious. A sheet wrung out of warm water is applied, as hot as the patient can bear it, over his whole body, including and closely embracing the limbs, and leaving no part of the person but the head uncovered. Over the sheet several blankets are tightly wrapped, or "packed," after the fashion of the hydropaths, but without the slightest respect for their pathology, or wish to imitate what they can with justice claim as their exclusive practice. Between the folds of the blankets, vessels full of warm water are disposed at intervals. The patient is then placed in a position which enables him to vomit over the side of the bed, and is supplied with toast and water, hot or cold, *ad libitum*. The remedy is an ancient one, often revived in modern times, and is to be regarded merely as a simple and powerful hot-bath. Whether it acts by restoring the healthy functions of the skin, by preventing evaporation, or by conveying fluids into a system from which they have been previously drained away, may possibly admit of question. It certainly seems to me, when applied in the case of children suffering

from the collapse of cholera, to be a most valuable and rapid mode of restoring the natural temperature. I have seen reaction established in a bad case within two hours after the application of the sheet. It is, however, generally necessary to continue the use of the remedy for six or eight hours. This practice seems less applicable to adults; the extreme restlessness, jactitation, efforts to vomit and to procure drink, usually observed in such patients, render it quite impossible to continue the application of the sheet for more than a few minutes at a time, without more constant nursing than the utmost vigilance on the part of the medical attendants can, in an hospital, insure. Strong patients commonly succeed, ere long, in disengaging their arms, and in throwing the bed-clothes off the upper part of the trunk, thereby exposing an extensive moist surface to evaporation, and totally defeating the object which we seek by the use of the sheet to attain.

Chloroform inhalations, it was not unnatural to suppose, would be found of considerable efficiency in at least removing the painful sensations of the patient during an attack of cholera. Most marvellous accounts of their efficacy in removing the most formidable symptoms of cholera have been published, and most unaccountable it seems to me, that in a large number of experiments undertaken while strongly prepossessed in favour of the remedy, conducted with considerable patience, and all the precautions conceived necessary to insure success,—conducted moreover in a public hospital, and before the eyes of many observers of acknowledged accuracy,—I should have totally failed in achieving similar delightful results. Truth compels me to state, that although chloroform has done much for the *comfort* of my cholera patients, it has done nothing for their *cure*. The cramps and urgent vomiting cease under its use, but recur whenever the patient awakes; and, although the soporific influence be maintained for hours by repeated inhalations, the result is still the same. The acts of vomiting and purging cease for a time, but the fluids are nevertheless still collecting in the stomach and bowels; the pulse becomes smaller and smaller, till it finally ceases to be felt at the wrist; the respiration becomes slower, the temperature of the surface sinks, as in too many cases of fatal collapse, and death closes the scene. I confess it seems to me probable, that chloroform inhalations, when administered during a profound state of collapse, may precipitate the fatal event by diminishing the frequency of the respiratory movements. While I deny the drug all claim to the title of a specific in confirmed cholera, I do not deny its efficacy in removing the earlier and more painful symptoms of the disease, nor decry the propriety of using it at any period when the cramps are severe.

The Russian physicians have strongly recommended the Persian Petroleum, and many trials of this substance have been made in the cholera hospital. The authenticity of most specimens of the drug to be met with in commerce, may with reason be suspected; but by the kindness of Dr Christison I have been enabled to use a sample

which is undoubtedly genuine. When given in doses of ten or twelve minims, immediately after the contents of the stomach have been ejected, either with a tea-spoonful of Tincture of Cardamoms, or suspended in mucilage, I have frequently found it of service in preventing the recurrence of *vomiting*, and believe that when this symptom is troublesome, the remedy is, at least, as certain as opium, acetate of lead, or calomel. In the *majority* of cases it has (like every other drug) failed, and certainly it has no specific action as a cure for cholera.

The injection of a saline fluid into the veins has, by some practitioners, been so strongly recommended, and, on theoretical grounds, seems so rational a practice, that I have not scrupled to resort to it in eight desperate cases. It has never seemed to me a sufficient objection to this practice, that a very large proportion of patients subjected to it die; and had I succeeded in saving a single individual of the eight, by injecting the veins, I should have thought myself fully justified in repeating the experiment. For of these eight cases, not one, according to all human probability, had the slightest chance of a prolonged existence, unless it were by the application of some remedy equally prompt and energetic in its action. Before the hospital in Surgeon Square was opened, the saline injection of the veins was thrice attempted in the Royal Infirmary; in one instance, I believe, with temporary benefit; but all three patients died within a few hours after the performance of the operation. One of these patients, an old woman in the last stage of collapse, fell under my charge, and had five pounds of the ordinary saline solution, recommended by the late Dr Mackintosh, injected cautiously, at a temperature of 98°, into the median basilic vein. The process lasted about forty minutes; the patient did not rally in the slightest degree, and died in my hands. It was not unreasonable to infer, that the sudden death in this case was the immediate consequence of the operation; yet dissection satisfactorily showed that *no air* had entered the heart or veins. In the eight cases treated in Surgeon Square, the solution used consisted of Muriat. Sodæ, ʒss., Bicarb. Sodæ, ʒiv., and Phosph. Sodæ, ʒss. dissolved in ten pounds of water, and then filtered. Care was taken to keep the solution at a temperature of 110° to 115°, and all reasonable precautions were adopted in the performance of the operation. The instrument used was an ordinary Read's syringe, and it was carefully cleaned before and after each injection. If this precaution is not observed, a quantity of *copper* must be injected along with the first portions of the fluid. A little difficulty was sometimes experienced in avoiding a certain degree of injection of the cellular tissue of the arm. The best mode of preventing this accident is to puncture the vein as in ordinary venesection, without any preliminary dissection, to push the silver tube into the vein gently, until the cross plate rests accurately upon the skin, and, above all, not to hurry the process of injection. In only two of my eight cases was even temporary amendment observed,—all died within fourteen hours. The

quantity of fluid injected varied from two to five pounds. These results have been sufficiently discouraging, and have led me seriously to consider if a repetition of the practice is advisable. Cases of the most unpromising aspect do occasionally rally, and get well after a collapse so profound as to leave no reasonable hope of recovery. I may instance a young woman named Hutcheson, whose case seemed quite desperate, and to admit of no rational remedy but injection of the veins. *No suitable vein could be found*, yet the patient is now quite convalescent. Are unexpected recoveries like this more frequent than cures effected by the saline injection of the veins in equally desperate circumstances? Till this question be resolved, I think we may still occasionally attempt a cure by the operation.

The treatment applied to the different stages of the disease, has been briefly as follows. In the earliest stage a mustard-emetic is given, followed by the use of the warm foot-bath, by full doses of opium with calomel, and the application of dry heat in bed. I fear it is not usually possible to cut a case of cholera short, even when its early symptoms are attended to; certain it is, that the above treatment failed in the cases of three hospital nurses already referred to. Another nurse, who had been vomiting and purging at intervals for some hours, had her symptoms promptly relieved, and was subsequently rather severely salivated by the calomel taken; I am however not satisfied that her case was one of cholera. When the early symptoms are unequivocal, and especially if the cramps are severe, a full blood-letting is practised, provided the pulse, age, &c., of the patient do not counter-indicate the remedy. The bleeding is followed by full doses of opium, with or without calomel. In by far the greater number of cases brought to hospital, collapse has already come on. In this stage the application of dry heat, frictions with turpentine, sinapisms to the epigastrium and legs, the moderate use of stimuli, and occasionally the hot wet sheet, are chiefly trusted to. In these different stages of the disease, a number of other remedies are from time to time used to palliate certain symptoms, *e. g.* chloroform, petroleum, calomel, opium, acetate of lead, tannin, and other astringents. Enemata, containing acetate of lead and opium, are occasionally thrown into the rectum. From the earliest period of the disease, I have usually prescribed a mixture containing camphor, cardamoms, and nitric æther, and the hourly use of some saline powder, such as Stevens', with abundance of diluent drinks. These remedies may be useful as stimulants, as diuretics, and as likely to supply to the blood some of the important constituents of which, during cholera, it becomes so rapidly deprived. I must add, that, although in deference to the experience of other physicians, I have given calomel to most of my patients and in various doses, I have never seen any good effect result, which might not be fairly attributed to the opium with which it was combined. The *scruple* dose of calomel has seemed quite inert, when given with the view of checking vomiting. During the stage of reaction the saline powders

and diuretic mixture have been always employed; opium is at this period either altogether abandoned or very cautiously given; moderate doses of *Ol. Ricini* have been found of service.

The return of the urinary secretion is always looked forward to with much anxiety; it seems in general the first trustworthy symptom of convalescence. When urine is long suppressed, the patient passes gradually into a drowsy state, accompanied with slow respiration, with a peculiar noisy delirium and jactitation, with rigidity of the extremities, grinding of the teeth, sometimes with general convulsions, and terminating in death by coma. Analysis of the blood in these cases shows the presence of *urea* often in considerable quantity. The first urine excreted during convalescence often contains albumen, but in a few days this principle can no longer be detected. The amount of *urea* in the earlier portions of urine is but small; and as this substance is afterwards more abundantly eliminated, the patient returns to his former state of health. There seems consequently a strong indication for the use of diuretics at all periods of the disease, and more especially about the period of reaction. The well-known property, possessed by colchicum, of increasing the amount of *urea* in the urine, suggests its employment when the comatose symptoms commence to manifest themselves, and I trust belong to be able to report upon its efficacy.

16th Nov. 1848.

Part Second.

REVIEWS.

Leçons sur les Phénomènes Physiques des Corps Vivants. Par C. MATTEUCCI. Paris: 1847.—*Lectures on the Physical Phenomena of Living Beings.* By CARLO MATTEUCCI, Professor in the University of Pisa. Translated under the superintendence of JONATHAN PEREIRA, M.D., F.R.S. London: 1848.

Elements of Natural Philosophy; being an Experimental Introduction to the Study of the Physical Sciences. By GOLDING BIRD, A.M., M.D., F.R.S., F.L.S. London: 1848.

THERE is not, perhaps, any branch of physical science to which we are more indebted for just conceptions of the phenomena of life, than to electricity. The great similarity between the effects of dynamic electricity or galvanism and the nervous influence, was made so irresistibly evident by the celebrated experiments of the philosopher of Bologna, that numerous investigations were set on foot to determine the identity of the two forces. The researches prosecuted for this purpose by Galvani, Volta, Aldini, Humboldt, and others, though they failed in elucidating the point towards

which they were directed, gave a considerable impulse to the study of animal excitability, and led to the discovery of a multitude of facts in the physiology of the muscular and nervous systems, to the knowledge of which it is probable we might never have otherwise attained. Having thus early become connected with the science of medicine, the introduction of galvanism as a therapeutical agent soon followed, and many wonderful accounts of its efficacy in this department of the healing art were published. Its application, however, was then, and still remains, in a great measure empirical; nor can we expect it to be otherwise, while the prosecution of electrical science continues to be so generally neglected by the physiologist and medical practitioner. It is almost impossible to turn over the pages of our medical literature without being struck by the strange blunders made by writers on electricity. Thus we have seen the action of nerve on muscular fibre explained by comparison with the helix of an electro-magnet. A high authority informs us, that the brain may be the organ of secretion of electricity, "a voltaic pile," on the principle of Zamboni or De Luc, "discharging itself at intervals along the course of the nerves which communicate with the nerves of the heart, and thus exciting its pulsation." In another place, we find a writer gravely describing the effects of a *current of electro-magnetism*, little dreaming that he might with as much propriety write of a current of hydrostatics, as "electro-magnetism" is merely a term expressive of a class of phenomena observed to proceed from the mutual agency of electricity and magnetism. Even the translator of Matteucci's lectures has, at p. 209, while Dr Pereira somewhat relaxed the vigilance of his "superintendence," placed a torpedo between the sole and cover of an electrophorus with the expectation of obtaining a spark from the fish. Many other instances might be adduced indicating the propriety of authors making themselves familiar with the laws, or at least the terms, of this science, on which they sometimes find it necessary to write.

We believe that a work on electricity, explanatory of its physiological and therapeutical action, in the different states in which it is developed by frictional, magnetic, electric, and voltaic apparatus, would, at the present time, be acceptable to the profession. The manual of Dr Golding Bird is perhaps the best guide we have to the study of physical electricity; but even in it we look in vain for clear practical directions as to the administration of the voltaic current, or the relative intensity of different kinds of batteries; and he still maintains, regardless of the researches of Professor Henry on induced currents, that the common electro-magnetic machine affords shocks both in the opening and closing of the circuit of the battery current. In Dr Pereira's translation of Matteucci's lectures, and the excellent chapter on galvanism by Dr Bird, will be found a fair summary of the present state of our knowledge with regard to electricity in its relations to physiology; and we avail ourselves of the

rich mine of facts they contain, to place a concise sketch of this department of science before our readers.

Electro-physiology may be most conveniently studied in two divisions, the first being confined to the description of phenomena attendant on the development of electricity by living animal structures; the second, to the effects produced on the same structures by electrical agencies, either generated in themselves, or derived from external sources.

For some time past, the application of the term "animal electricity," has been confined to the effects produced by certain fishes furnished with organs adapted to the transmission of the electrical discharge, through conducting bodies placed in contact with them. The researches of Matteucci, and others, have rendered it probable that the excitation of this force is intimately connected with the exercise of the nutritive function, especially with the chemical changes accompanying the active processes of destruction and reparation going on in the living muscular tissue. These researches have, to a certain extent, revived the doctrines formerly enumerated by Galvani and Valli, who held that the well-known contractions which occur on making a communication between muscles and their nerves by metallic arcs, were due to the accumulation and discharge of a peculiar fluid existing in the organs themselves, and ascribed to the exterior of the muscle a negative, to the interior and its contained nerves a positive, state of electricity. Galvani, soon after the promulgation of his first experiments, observed that contractions were caused in the leg of the frog by merely bringing the muscles of the limb in immediate contact with the shoulder, after having removed all connexion between the upper and lower extremities except the sciatic nerves; and it was afterwards established by the researches of Humboldt, Aldini, Pfaff, and Müller, that the same contractions occur whenever the trunk of the sciatic nerve, dissected and insulated from the limb, but still organically connected therewith by its branches, is brought into contact with the muscular parts which it supplies, either immediately, or by the aid of a substance capable of conducting electricity; the member thus both supplying the motive force, and constituting a galvanoscope capable of indicating its effects. So far it was manifest that a peculiar force resided in the neuro-muscular apparatus, the disturbance of which was attended with contractions in the muscular part of it; but the proof, that such force was really identical with electricity, was reserved for the researches of Nobili. This philosopher observed, on placing the posterior extremities of a frog, prepared as by Galvani, in two glasses of water, so that the lumbar nerves were immersed in one while the legs occupied the other, that the galvanometer indicated the circulation of a current directed from the legs to the nerve, when the extremities of its wires were plunged into the glasses. M. Matteucci, who has confirmed and extended

the experiments of Nobili, succeeded in augmenting the intensity of the current, by employing, instead of a single animal, a pile or battery formed of the extremities of a number of animals; this was effected by causing "the nerves of the first pair of extremities to touch the legs of the second, and the nerves of the second the legs of the third, and so on." He has further ascertained that the current thus generated, which had derived the appellation of "the proper current of the frog," from its having been previously observed in that animal only, may be detected in all other animals.

A current, apparently distinct from that already described, has been detected by M. Matteucci circulating from the exterior to the interior of the muscles, to which he has given the name of the "muscular current;" it may be readily observed by making an incision into the muscle of an animal, alive or recently killed, and conveying the poles of a very delicate galvanometer, the one to the surface of the muscle, the other to the interior of the wound. When a galvanometer is not at hand, the prepared frog's leg may be employed in its place; for this purpose, the muscles and bones of the thigh and pelvis are to be removed, the nerve left as long as possible, and the limb enclosed in a tube of varnished glass. On drawing the nerve across the edge of an incision made in the muscle of a living animal, the "galvanoscopic frog" will contract whenever the nerve touches both interior and exterior of the muscle.

A pile may be formed of the muscles of frogs' thighs, by dividing a number of them in the middle, and alternately connecting their exterior and interior surfaces; the muscular current from such an arrangement increases in intensity with the number of alternations. M. Matteucci has thus succeeded in producing, not only considerable electro-magnetic effects, but also the decomposition of solutions of iodide of potassium, and, with the aid of a condenser, deflections of the gold leaves of an electroscope.

In piles formed of the muscles of animals belonging to different classes, the current possesses an intensity greater as the animal ascends in the scale of being; its duration, on the contrary, is in proportion directly inverse. Thus the weak current generated by the muscles of fishes and reptiles is perceptible for several hours after death; whilst the comparatively powerful one, derived from the muscles of birds and mammals, cannot be detected after the lapse of a few minutes. A considerable influence is also exercised on the pile by the organic condition and temperature of its elements, muscles, the nutrition of which has been checked in any manner, or which have been submitted to a very low temperature (32 Fahr.), being capable of affording only slight and brief evidences of electricity. A similar deterioration of the muscular current is noticed in animals which have been destroyed by asphyxia, and still more so by sulphuretted hydrogen; while it seems to be unaffected in those poisoned by carbonic and hydrocyanic acids, arsenu-

retted hydrogen, or narcotics; in muscles which have been the seat of inflammation, or which have been taken from well-fed animals, the muscular current possesses more intensity, and continues for a longer period.

M. Matteucci is led to believe that the muscular current is independent of the nervous system, from his having formed piles equally efficient with muscles deprived of their nervous filaments with the utmost care; and he considers that its excitation is entirely due to the changes of electrical equilibrium accompanying the chemical actions of the nutrition of the muscular fibre,—that these in the living organism being molecular, it is impossible for the electric current so excited to *circulate* in the muscular masses in the natural condition of the parts; and it is only when the interior of the muscle is placed in artificial communication with the surface, which does not suffer the same chemical action, that any evidence of the existence of a current can be observed. M. Matteucci is also of opinion that the “proper current” has a community of origin with the “muscular current,” from his having ascertained, by removing the thighs and spinal nerves from the elements of a pile adapted to the generation of the “proper current,” that it was equally powerful, though only derived from the tendinous muscles of the leg; and further, he discovered that in all muscles which terminate in a long thin tendon, as the gastro-enemii of frogs, and the pectoral muscles of birds, a current circulates directed from the tendinous extremity to the muscular surface. Now, it has been demonstrated by Bowman and other anatomists, that the tendon becomes continuous with the elementary muscular fibres at the point where the sarcolemma ceases to invest them; the tendon and muscular fibre may, therefore, be in the same electric condition, and a communication made between the former and the sarcolemma probably constitutes a medium of conduction to the “muscular current.”

Electric currents have been detected in living animals by MM. Favio and Zantedeschi, passing from the cutaneous tissue to the cerebro-spinal axis, which they have called “the electro-vital external currents,” and others, “electro-vital internal currents,” directed from the cerebro-spinal axis to the organs situate beneath the skin. They did not succeed in obtaining evidences of the connexion of electricity between different parts of the same viscus, or from viscus to viscus; but M. Matteucci has observed a considerable deflection of the galvanometer, by applying its wires to plates of platina, the one placed within the stomach, the other on the surface of the liver of the rabbit. On dividing the nerves and vessels as they passed through the diaphragm, the needle receded from 20° to 3° , and retired to its normal position, when the head of the animal was suddenly struck off. M. Favio and Zantedeschi assert, that pain and weakness have the effect of suspending the electro-vital currents, and in some instances of altering their direction.

Hitherto we have considered the production of electricity chiefly by the nutritive processes of the muscular tissue, as rendered manifest by purely artificial arrangements; we have now to direct the attention of our readers to phenomena accompanying the development of the same force by animals naturally furnished with a peculiar apparatus for that purpose—electric fishes. This department of animal electricity has been most successfully investigated in the several varieties of the torpedo and the gymnotus electricus.

The electrical organs of the torpedo are composed of numerous prisms, closely compressed together in flattened masses resembling honeycombs, and occupy the whole thickness of the fish on either side of the head and branchiæ, the bases and apices of the prisms corresponding to the animal's lower and upper surfaces. Each prism is formed of a single row of superimposed vesicles, containing a dense albuminous fluid, and is divided by numerous horizontal septa where the walls of the neighbouring vesicles are in contact, while between the two layers of the septa thus formed, there is extended a network of nerve tubules, repeatedly anastomosing with each other, derived from ramifications of the trifacial and pneumogastric nerves, distributed over the walls of the prisms. The organs in fact consist of a congeries of vesicular bodies, each of which is an elementary electrical apparatus, perfect in itself, and capable, when removed from the prism to which it belongs, of manifesting the signs of discharge. M. Matteucci has frequently caused contractions in the "galvanoscopic" leg by the application to its nerve of a portion of one of the organs not larger than a pin's head. Now the number of prisms in the organs of a large torpedo is estimated by Hunter to be about 1000, each of which contains 150 vesicles; if, therefore, we believe with M. Matteucci that the discharge is proportional to the number of vesicles, we can easily understand how the current acquires the high intensity which it possesses. We think, however, that the intensity of the current ought, judging by analogy with the effect of the number of alternations in the voltaic battery, to be proportional to the number of vesicles in each prism, rather than to the whole number contained in the organs.

Our previous ideas of the excitation of electricity, give us little assistance in arriving at a conclusion as to the mode in which it is effected in the organs of the torpedo. We find it difficult to understand how a mass of nervous and muscular matter can either act as the generator of a current, or as the condensing receptacle of electricity derived from the nervous system. M. Matteucci suggests that the vesicles are thrown into a state of polar tension by the nervous influence, in a manner analogous to the polarization of the tourmaline by heat, an hypothesis which, though one of considerable ingenuity, is not quite original, as a somewhat similar suggestion may be found in the *Cyclopædia of Anatomy and Physiology*, vol. i. p. 54.

The function of the electric organ is entirely dependent on the integrity of the nerves supplying it and the centres from which they arise, which appear to be the ganglia of the vagi and trifacial nerves, so largely developed as to have obtained the names of the electric lobes. By removing the whole of these parts from the animal, and carefully preserving their connexions with each other, it is proved that the integuments and cartilages by which they are surrounded in no way influence the discharge; it continues after the organ has been sliced and pierced in various directions, and it is only by the gradual death of the part, or the destruction of its chemical constitution by the application of heat or acids, and the consequent coagulation of its albumen, that its peculiar manifestations cease to exist. On the other hand, the section or ligature of any of the nervous branches is attended with instant *paralysis* of that part of the organ to which it is distributed; and any lesion of the electric lobe, after causing violent discharges, is followed by complete destruction of the electrical function.

The effect of voltaic currents on the nerves is very remarkable, as showing the analogy existing between the discharge and muscular contraction. When the poles of a battery are applied to the nerves of an organ recently removed from the animal, a discharge takes place both on opening and closing the circuit, independently of the direction of the current through the nerve; but when the vitality of the parts is weakened, the shock occurs only on the closure of the circuit, when the current is made to traverse from the cerebral to the peripheral part of the nerve, and on the interruption of the current, when the electricity is passing in the opposite direction. By referring to the action of the current in mixed nerves, it will be found that muscular contraction takes place under similar circumstances.

The current of the torpedo is capable of producing all the usual effects of voltaic electricity, viz. the heating of metallic wires, chemical decomposition, the excitation of magnetic polarity in ferruginous bodies, and the phenomena of the electric spark and of muscular contraction. During the discharge the whole of the opposite surfaces of the fish are thrown into a state of electric polarity, the dorsal surface becoming positive, the ventral negative; but the galvanometer indicates that the poles or points of greatest activity correspond to the opposite surfaces of the electric organs, the apices and bases of the prisms; weak currents may, however, be detected passing between any two parts of the integuments, differing in their respective distances from the organs, and even between different parts of the organs themselves.

The electric organs of the *gymnotus* consist of masses of prisms similar to those of the torpedo, but having a direction coincident with the long axis of the animal's body: hence the poles are situated at the head and tail,—the cephalic pole being positive with regard to the parts of the body situated towards the caudal ex-

tremity. The current of the gymnotus differs from that of the torpedo only in the possession of a much higher intensity, due to the greater length of the prisms contained in the organs of the fish, and the consequently greater number of vesicles polarized in series. We cannot but receive with some hesitation the statement of Faraday, as quoted by M. Matteucci, that the shock of the gymnotus is equal to a highly-charged battery of Leyden jars, having an united surface of 3500 inches; the discharge from such an apparatus would fuse a considerable length of iron wire, a feat surely beyond the powers of the gymnotus.—(*To be continued.*)

A Course of Lectures on Dental Physiology and Surgery, delivered at the Middlesex Hospital School of Medicine. BY JOHN TOMES, Surgeon-Dentist to the Middlesex Hospital. London. 1848. 8vo.

WE have intended for some time past to notice this work, and to devote to it a space in our pages which we conceived to correspond with its great excellence, and the originality and importance of the author's views. As we find, however, that objects of more immediate interest have occupied our pages entirely for the present, we must be content with giving our readers a general idea of the style and scope of these lectures, which we have regarded, ever since their publication in the *Medical Gazette*, as a model of what such a course of instruction ought to be. As treated by Mr Tomes, the science and art of the dentist are made mutually to illustrate each other; and, in both departments, he is not only *au courant* with the present state of our knowledge, but prepared to impart to us many new facts and views, the result of patient study and long-continued observation. It is the constant enunciation, and equally constant practical application, of the highest truths of science, which gives to this work a tone so far superior to that of the majority of publications on practical dentistry.

The first two lectures are occupied with an account of the anatomy of the teeth and alveoli. The third, fourth, and fifth lectures give the results of his researches on the minute structure and development of the different tissues composing the teeth; together with the views of Mr Owen on this subject; and a summary of the researches of Mr Goodsir on the development of the pulps and sacs of the teeth. The investigations of Mr Tomes on the development of the dentine, and its analogy with bone, are exceedingly interesting; and we shall place a summary of them before our readers in an early Number of the *Retrospect*. The sixth lecture treats of the eruption of the temporary and permanent teeth; and concludes with some excellent remarks upon the adaptation of the teeth and hard parts of the mouth to their various functions:—

“In taking a retrospective view of the anatomy and physiology of the teeth, we are forcibly struck with the adaptation of these organs to their peculiar

functions. Early in life, when the jaws are small and comparatively powerless, the small and more delicate milk teeth are provided by nature, to perform the necessary office of mastication. When the body has increased in size, and the jaws have become larger and more powerful, the smaller teeth are, by a natural process, removed, and a stronger and more numerous set is developed, with which we are enabled to masticate the more solid food required for the sustenance of the adult.

"Like the bones generally, the teeth are required to support mechanical resistance, and in them, too, we find the structure admirably adapted to this end, but in the teeth the mechanical force is direct in its application; and in them we find a peculiar arrangement of osseous structure, enabling them to withstand without injury the force so applied. And, as a further adaptation, the teeth are endowed with a lower degree of organization than the bones, and are thence less susceptible of injury; while the latter, clothed with soft tissues, and capped with cartilage, are thus defended from the more immediate effects of mechanical force.

"The teeth are said to possess in themselves no power of reproduction by which an injury can be repaired. This is not strictly true. The injury to which, in a state of nature and health, they are most liable, is the wearing away of the masticating surface from use. The worn surface certainly is not renewed, but the teeth increase in density, and the pulp-cavity diminishes in size by the formation of dentine, so that the actual amount of dentine is not diminished, while the density is increased. In each of these actions we may recognise a form of renewal which in some degree compensates for the loss by abrasion. If the whole act of mastication is from any cause thrown upon two or three teeth, then these naturally, by the excessive use, wear away, till at last the whole crown is exhausted. Then, again, they make an effort to resist the inroad of caries, as I will show you when we come to treat of that destructive disease.

"*The teeth are important as organs of articulation*; so much so, that, when lost, we can scarcely make ourselves intelligible. The physiology of the teeth, when limited to this use, is well worth investigating, had we time at our disposal; but, before leaving this part of our subject, I may draw your attention to a few curious facts, in connexion with the form of the mouth, in those possessed of a good voice, and with power to use it in song.

"Wherever you have a fine clear sonorous voice, you will find well-formed and well arranged teeth; each tooth will occupy its proper place. But, what is perhaps still more important, the hard palate will be well formed; that is, it will present a section of a large arch, perfectly free from contraction either from side to side, or from before backwards. There will not be a deep vaulted form, neither will there be a sudden elevation immediately behind the front teeth, so common in those who speak with indistinctness—on the contrary, the palate will rise gradually.

"The mouth, and its dental appendages, are not of the first importance in relation to the voice, yet they are highly important as auxiliaries, and, as such, their condition should not be lost sight of.

"The vocal organ may be good, even first-rate, but the mouth and teeth must be well formed, or the voice will be injured in its passage through the mouth."

Lectures VII. to XIV. are on the diseases of the teeth, gums, and alveoli. The author's opinions on these subjects are very distinct and practical, and apparently the result of great experience. He has collected some valuable information on the liability of the different teeth to caries, by recording the cause of extraction in a large number of individual instances; and has thrown the results in the present work into a tabular form. In regard to the treatment of toothache we find the following suggestion:—

"Most of you know that an ulcerating or a suppurating surface sometimes becomes extremely sensitive and painful, and that immediate relief is obtained by brushing over the surface of the sore with a weak solution of nitrate of silver. A similar condition, I apprehend, now and then obtains with the exposed surface of the dental pulp, for we occasionally find that a solution of nitrate of silver, gr. iij. or gr. iv. to the ℥j. of distilled water, will allay tooth-ache after the failure of what at first seemed more promising treatment."

Lecture XV. is on the operations practised on the teeth; and Lecture XVI. (the last) is on artificial teeth. The author is averse to the use of chloroform in extraction of teeth, conceiving that the effects of the former are sometimes more distressing to the patient than the transitory pain of the latter. We conceive that the patient ought in this matter to choose for himself; and we have no hesitation in saying, that, for our own part, we should not be quixotic enough to undergo the pain of a wrench from Mr Tomes' instrument, while we had the means of annulling it.

We close our extracts with the following remarks upon stopping decayed teeth. After describing the mode of removing the softened dentine, he says,—

"Gold or tin foil are the best materials for making plugs. Whichever of these be chosen, the method of use is the same.

"There are four methods of introducing foil for making a plug. In one the metal is folded into narrow strips, proportioned in width and thickness to the size of the cavity. One end of the strip is, by means of a conveniently shaped stopping instrument, pressed to the bottom of the cavity. The strip is then bent, and a fold passed to the bottom of the hole, leaving the first fold projecting above the surface. Fold after fold is introduced, till the cavity is tolerably full. A wedge-shaped instrument is then introduced, and the gold pressed towards the walls of the cavity; more gold is, by a similar process, pressed into the cavity so obtained. This process is repeated till the wedge cannot be forced into the plug. A flattened instrument is then used to compress the gold in the cavity. When we can make no further effect on the surface of the plug by compression, the surface is filed smooth and burnished. By a careful adherence to this plan, we make a plug composed of layers of metal, arranged parallel to the walls of the cavity, and therefore not liable to fall to pieces or come out. But, on the other hand, had we made the folds at a right angle to the walls, and parallel to the bottom of the cavity, layer after layer would have peeled off, till little or none of the plug remained, and the decay would have proceeded to the destruction of the tooth.

"In the second method, a piece of foil of sufficient size is rolled hard, and spherical between the thumb and finger. This is gradually forced into the cavity, care being taken to get it well in round the outer walls. When the plug has been rendered as solid as possible, the superfluous portion is cut or filed off, and the surface burnished.

"The third method of using metallic foil is a combination of the two preceding ones. A piece of foil that will readily go into the cavity is rolled up loosely. When in its place, a wedged-shaped instrument is passed into its centre, which has the effect of spreading the gold towards the walls of the cavity. The centre is gradually filled with folds of gold in the manner I have described. The wedge is used again and again, till it can no longer be made to enter. The gold is then compressed on the surface, and the superfluous portions removed, and the surface burnished. When the plug is finished in either of the manners I have described, the circumference should be examined by a sharp steel probe. If this can be made to enter at any part, the hole so made

should be enlarged by thrusting in an instrument as large as can be introduced, and the hole filled.

"In the fourth method of plugging, the foil is rolled into short lengths, proportioned to the depth of the cavity to be plugged. These, with the assistance of a fine pair of forceps, are packed into the tooth, much in the manner you would proceed to pack cigars into a tumbler. A wedge-shaped tool is from time to time thrust between the length of the foil, to force them towards the walls of the cavity. When the tool can no longer be made to enter, the surface of the plug is cut level with the surface of the tooth, and burnished.

"Either of the foregoing methods of plugging will answer, if well done. But, of these, I prefer introducing the metal in folds. The situation of the cavity, and also the size, will have something to do with the selection of the plan of operating. Then, again, one person will be more apt at one manner of procedure than another. All these matters of detail must be learned in practice."

The work is very handsomely printed, and the illustrations are admirable.

Report of the Proceedings of the Pathological Society of London.
Second Session, 1847-48.

WE are happy to welcome a second fasciculus of the Reports of this most important and useful Society; and still more happy to be enabled to say, that it appears to be gaining materially in strength as it proceeds. We can well imagine the difficulty of bringing a popularly constituted society such as this into good working order; a difficulty arising quite as much from the enthusiasm of some of its members, as from the lukewarmness of others. It would be too much to expect that so young an association should be able from the first to temper the zeal of all its members with that discretion which is only the fruit of experience, or raise the scientific character of its contributions to the level that might justly be expected from an older or a more exclusive institution. But we are persuaded from the signs of life which it has already shown, that the Pathological Society will go on improving in this respect from year to year; and when we consider the vast materials which it has at command, we cannot doubt that its transactions will become one of the greatest repositories of important pathological facts and doctrines in this country, or in the world.

In order, however, that this desirable end may be accomplished, it is above all things necessary, that the facts which occupy the attention of the society should be collated and discussed with a view to the elimination of *principles*; and that they should be placed on record in such a form as shall tend to make them useful, not only to those who have seen the objects described, but to the medical public at large. These desiderata can only be accomplished, as it appears to us, by a right understanding on the part of the society of their importance, and a resolution to throw large powers into the hands of a committee of the most able and intelligent of their mem-

bers. These powers should extend to the judicious exclusion of what is unworthy of publication, and the correction or completion, in every possible case, of what is erroneous or defective in description while the objects described are still accessible. That such an authority does not exist at present, we conclude from the note appended by the council to the present part of the reports, declaring that they have proceeded in a great measure upon an opposite principle. And we think that any candid member of the society will readily admit, that the volume already published has probably suffered much from this fastidiousness. It would be invidious to particularize instances; but so long as we find important objects described, as they are in numerous cases, by *mere names*, without any reference to those physical characters of form, colour, size, &c., which alone can convey real information to those who require it; so long as we observe the most careless and flimsy productions occupying the same honourable place in this volume with those that are really the gems of its pages,—we must believe that a careful and judicious superintendence is a desideratum which the society will do well to supply, if it is to perpetuate its own best interests and highest usefulness.

The fact is, that while the power of detailing symptoms and treatment belongs to *many* medical men, the art of pathological description is possessed by very *few*, being both somewhat difficult of acquisition, and very much neglected by the majority of practitioners; and hence it happens, that in very many reports on pathological subjects, the glaring errors and obvious deficiencies are such as to defeat any useful purpose. It should be the care of such a society as this, that none of the important objects submitted for its examination are lost to the public through inadequate description; and it ought, therefore, to be the *duty* of the council to make themselves responsible in some degree for the reports, and by judicious retrenchment, addition, and emendation, to place them in conformity with the specimens presented, and with the demands of the age. In this way the society will become a source of instruction to its own members, as well as of profit to the public; and while it has the power of enrolling in its council, as at present, a large number of names, honoured as those of skilled and tried labourers in the path of scientific inquiry, and in particular of pathological description, we think that no fear need be entertained, that the duties in question would be exercised with delicacy and discrimination.

If the expression of our opinion in this matter, shall have in any degree the effect of inducing the members to confer such an authority, or of strengthening the hands of the council in using the powers they already possess more effectively, our object as critics will be accomplished; and we are satisfied that in saying this, we speak the wishes of many who look to the Pathological Society of London as one of the most promising institutions of the present day.

The section, both in this number and the last, on the malformations of the heart and great vessels, contains a number of most in-

teresting and rare anomalies, and mostly very well described. The greater number bear on the question of *cyanosis*, and they appear to us in most cases to correspond, on the whole, with the views advocated by Dr Mayne of Dublin (see *Retrospect* for June 1848, p. 110); but we have not space at present to enter on an analysis of them. Perhaps the most curious, are a case by Dr Chevers, of congenital obliteration of the pulmonary artery, with nearly complete closure of the *ductus arteriosus*; and another by Dr Peacock, of absence of the same artery, the aorta arising from the right ventricle, and giving off the pulmonary branches through the *ductus arteriosus*.

The most interesting contributions to the pathology of the other organs, are a case of epithelial cancer of the tongue, excised, and apparently cured, by Mr Critchett; a case, by Dr H. M. Hughes, of perforating ulcer of the stomach, communicating by a fistula with a gangrenous cavity in the lung; some curious details by Mr Prescott G. Hewett, tending to show the frequency of ulceration of the duodenum in cases of external burn; and some excellent observations by Dr Handfield Jones on a form of dysenteric ulceration. The same author has a note on the scrofulous deposits found in the ureters, showing that they are probably deposited subjacent to the mucous membrane. Dr Williams presented a kidney from a gouty person who died of phthisis, in which the tubes contained crystals of urate of soda, which was also deposited in the joints. Unfortunately, there is no description of these crystals, or of the case, which would have been very desirable, and could have been easily supplied.

We heartily wish the Pathological Society of London success, and hope that, as that success becomes confirmed, every new volume will show increased care and vigilance on the part of the council to reform all imperfect description, and to encourage well-regulated discussion and generalization.

The Periodoscope; with its Application to Obstetric Calculations and the Periodicities of the Sex. By W. TYLER SMITH, M.B., Lond., Obstetric Lecturer in the Hunterian School of Medicine.

WE have before us a small book, containing a table or dialplate, designed to assist the practitioner in making obstetric computations, by converting for him, without need of calculation, calendar into lunar months. This is accompanied by remarks upon the different alleged periodicities of the female sex.

The table itself is a neat and ingenious expedient, which may be useful for class-teaching, or in the wards of a maternity charity. It must, however, be always borne in mind, that the great difficulty in prognosticating the duration of pregnancy, and the periods of danger during pregnancy, lies not in the calculation of days, but in the determination, as near as possible, of the period

of conception ; and further, that at best the results are only probable.

To one who has not the faculty of easily dealing with numbers by a process of the mind, the periodoscope will be a valuable aid ; but to an experienced obstetrician, accustomed to the calculations which the periodoscope supplants, it will not prove of much service. Dr Smith's theoretical remarks upon the periodicities of the sex are well worthy an attentive perusal ; but we doubt if they can be deemed of much practical value.

Physiological, Anatomical, and Pathological Researches. By JOHN REID, M.D., Fellow of the Royal College of Physicians of Edinburgh, Chandos Professor of Anatomy and Medicine in the University of St Andrews, &c. &c. Edin. 1848. 8vo, pp. 659.

FEW names stand so high in experimental physiology as that of Dr John Reid. His labours in this department have always been distinguished by a completeness and perseverance which went far to exhaust every branch of the subject, and leave the path of inquiry for a considerable time barren to future investigators. Of this kind were his experiments on the eighth pair of nerves, and on asphyxia ; notwithstanding that eight or nine years have passed since their publication, these subjects still remain *his own* ; nor have the subsequent investigations of Volkmann and Valentin, or of Mr Erichsen, availed to deprive him of the honour of having thoroughly investigated every branch of these subjects, so as to make his researches the basis for all others. And although, from the period of rapid transition through which the anatomical and pathological sciences have passed of late years, he had not been able to monopolize any similarly exclusive domain in these departments, still his researches on the medulla oblongata, heart, and placenta, and his reports on fever, will long be regarded as standard authorities on these subjects.

The secret of his success, we think, lies chiefly in two circumstances ; in that clear and truly practical character of mind, which enables Dr Reid at once to apprehend where investigation is most needed, and what character it should assume ; and also in the pains which he invariably takes to form a candid estimate of the labours of his predecessors in the inquiry into any subject. The latter characteristic (which was well exemplified in the admirable history of the subject prefixed to his paper on asphyxia) fitted him admirably for the office of critic ; and we may be excused for bearing testimony in this place to the advantages which we have ourselves derived, at different times, from his co-operation with us in this branch of our editorial duties. We have great pleasure in seeing two of these anonymous contributions to medical literature enrolled in the present volume ; we allude to the reviews of Wattmann on air in the veins, and Burrows

on the cerebral circulation. In the last of these, a question which had become peculiarly embarrassed by imperfect conceptions on all sides, and positive errors of reasoning on the side of Dr Burrows, was discussed by Dr Reid in so clear and satisfactory a style, and was so plainly shown to require for its solution only an acquaintance with the simplest principles of natural philosophy, that we cannot help wondering how any one could have been deceived, and still more, that the fallacies of Dr Burrows should, in several instances of late, have been reiterated, and the peculiarities of the cranial circulation denied, by writers of reputation and merit, who cannot, we believe, have been acquainted with the complete refutation which these fallacies had received. We hope that all persons in future writing on this subject, will refer to Dr Reid's book, or to the original review in this Journal for August 1846.

In the present volume, all the more important of Dr Reid's contributions to science have been collected; and the author has been at the pains to append to most of them remarks, bringing down not only the results of his own experience, but also that of others, to the present time. In the case of the inquiry on the eighth pair of nerves, this addition extends to thirty-seven pages, and gives a most complete history of all that has been done on the subject. In the appendix to the article on asphyxia, he explains some differences between Mr Erichsen's results and his own, depending on a peculiarity in the form of the hæmadynamometer used by him. He dissents from Mr Erichsen's theory of the pulmonary stasis in asphyxia, and reasserts his own, which is the same as Dr Alison's—that it depends on the cessation of the chemical changes on the blood in the capillaries. We confess that we have never been able to see how the existence of these changes should advance, or their cessation retard, the progress of the blood; we should rather expect a contrary effect. On the other hand, it is most clear and intelligible, that a contraction in some part of the vascular channels would have the effect in question; and although we do not feel called on to assert this as positively the cause of the pulmonary stasis, and still less to adopt Mr Erichsen's view on contraction of the veins (which is, we think, pretty well disposed of by Dr Reid), yet we by no means think it proved that such contraction may not exist in the capillary vessels of the lung. It is not enough to say that capillaries have not been *proved* to contract; for as there are several very strong reasons for supposing that this is the case, and no objection worth mentioning has ever been urged against their contractility, we are disposed in the mean time to adopt this view, as the explanation of many phenomena very difficult of solution on any other hypothesis. We must admit, however, that Dr Reid's arguments, in favour of the power of the capillaries to propel the blood independent of contractility, are among the most ingeniously stated that we have seen.

Two new articles are added; the one, on sensational and emotional reflex actions; the other, on the value to be attached to experiments

on the nervous system. Both are sound and good; in the latter, the author defends the validity of such experiments against the indiscriminating aspersions which have been thrown on them, while he clearly shows the sources of fallacy to which they are liable.

The articles in this volume are twenty-seven in number. It is illustrated by plates and wood-cuts; and, from the whole style of its execution, is one of the most elegant, as well as useful volumes for a medical library which has ever emanated from the press of Edinburgh or London.

Clinical Lectures, delivered in the Theatre of Mercer's Hospital, during the Session of 1847-8. By JAMES F. DUNCAN, M.D., T. C. D., Assistant Physician to the Hospital, &c. &c. Dublin, 1848. 8vo, pp. 122.

THE lectures, which are here collected into a volume, were published during the spring and summer of the present year, in the *Dublin Medical Press*. The author appears to enjoy considerable reputation as a clinical teacher, and is also a lecturer on the theory and practice of medicine in Park Street School. As a specimen of the clinical teaching of the present time in Dublin, his work might have been critically compared with the excellent lectures of Dr Graves, which now lie before us in an improved second edition by Dr Neligan, and which may be considered to represent the older and more experienced school of Dublin physicians; but this would scarcely be just to the present author, whose light and occasional sketches of disease in this volume, delivered *viva voce* and scarcely re-touched or elaborated in any material degree, bear the impress of quite a different object and character from the large and almost systematic work of Dr Graves. We shall therefore delay noticing the latter till next month, when we hope to accord to it a space commensurate with its importance.

The lectures of Dr Duncan are written in an easy and flowing style, free from mannerism and obscurity, and in every way well adapted for instruction. To the matter of them we have already been indebted in the pages of our *Retrospect*, having read some of them with interest and approbation in the form in which they were originally published. In others we find many objectionable statements, arising sometimes from too great eagerness in adopting the views of others, and sometimes from errors of observation; to some of these we shall presently allude. Where his data are sound, the author's conclusions are generally just and apposite, and moreover unwarpd by that spirit of exclusive nationality which finds its way into so many Dublin productions, covering many excellencies with a garment of outworn prejudice, by which nature and decency are alike outraged.

Lecture first is on diarrhoea, and contains nothing remarkable

excepting the strong recommendation of strychnia as a remedy. He conceives this medicine to be peculiarly, or rather exclusively applicable to the cases in which the bile is deficient in the evacuations; and ascribes its action to a power of restoring the secretions, and particularly the bile, to their natural quantity. He therefore recommends strychnia as useful, precisely in the class of cases in which opium fails.

Lecture second treats of several chest affections—emphysema, bronchitis, mitral and aortic valvular disease. In treating of the diagnosis of aortic disease by the pulse, he directs attention to the effects of posture.

“In the erect position—that in which most persons happen to be placed when the physician pays his visit—it is full, and hard, and strong; in the recumbent, on the contrary, it is weak and compressible: the reason of which I believe to be this—In the erect posture, the blood, being acted on by gravity, is forced down into the principal trunks, especially from the carotids; the vessels hence being tense and rigid, the pulse acquires a fictitious strength, which disappears as soon as the mass is more equably distributed by the patient assuming the horizontal position. Perhaps there may be a second cause operating to produce this result. The ventricle having to propel the blood in the erect posture against the force of gravity, by an instinctive effort contracts with greater vigour than at other times, when it has not this disadvantage to contend with.

Now there is some truth and much error in this statement. The *truth* is, that the pulse in aortic disease is full, bounding, and—more especially—*visible*, in proportion as the wrist is elevated, and the blood gravitates from it towards the body; for the very simple reason, that this peculiar character of the pulse depends on the diminished tension of the arteries, and is increased by every thing which diminishes the fluid pressure within them. The *error* is in assuming that the aortic pulse is ever in any posture *hard*; and as this is asserted repeatedly in the course of the lecture, we presume that Dr Duncan has not distinguished with sufficient accuracy between strength and hardness of pulse; the fact being, that however full and strong, the aortic pulse is *always* very compressible. We need not criticise the explanations given by Dr Duncan; indeed we cannot understand their application.

The *third Lecture* is on chlorotic anasarca, of which it gives a very good account.

Lecture fourth is on spinal arachnitis, an uncommon disease in this country, but which has been observed to occur in an epidemic form both in Ireland and on the Continent. The distinction of the disease from hysteria with spinal irritation—to which it bears considerable resemblance, from the tenderness of the back and abdomen, and the vomiting—is placed by the author in the acute febrile character of the inflammatory disease, and in the urine, which, being of high specific gravity, and often loaded with lithates, is very different from the pale limpid and watery urine of hysteria.

The *fifth Lecture* is on Bright's disease of the kidney, a disease

which the author seems to think has passed from obscurity into light, in consequence of the discovery that fatty granules in the urine constitute its only real sign. We recommend him not to trust to any such test, but to stick to his test-tubes and urinometer, until he has some more consistent and sure microscopic phenomenon. We beg to assure him that he will fail, in a majority of instances, to find oil granules present at all; and further, that if he finds them present, they are not to be trusted as unequivocal signs of the disease in question. The treatment is a great source of congratulation to Dr Duncan. Since the true nature of Bright's disease has been discovered to be fatty degeneration of the kidney, it has become "easy, intelligible, and simple; and, though we may not anticipate success in every instance (!), we can clearly perceive that there is nothing in the nature of the affection to render it necessarily incurable or fatal." Is it possible that Dr Duncan can think such a violent change of our views, and such sweeping assertions, practically warrantable in the present state of the pathology of this disease, without careful and repeated trial? To our mind, this whole lecture is most crude and unsatisfactory, and not the less so that we do not believe the data on which it rests to be well founded.

The next lecture—the *sixth*—is by many degrees the best in the volume. It relates to a case of pneumonia affecting the upper lobe of the right lung, with emaciation, and some of the general characters of phthisis. The diagnosis is very well and clearly stated. As we have been obliged to condemn the preceding lecture, we have much pleasure in quoting the following excellent and compact description of acute phthisis:—

"In its general aspect, acute phthisis resembles an attack of typhus fever; there is great disturbance of the system at large, and well-marked pyrexia. Often the attack is ushered in with a distinct and severe rigor; often it succeeds to the ordinary causes of common fever, such as exposure to cold, a heavy wetting, or sitting in damp clothes. In all cases the skin is hot, the pulse quick, the appetite fails, thirst becomes developed, and there is great prostration of strength. Still a careful observer would detect at once that it differs from real fever, in the exemption of the nervous system from participation in the general disturbance; the intellect is clear, the eye bright, there is no headache nor stupor, and the patient's repose is comparatively undisturbed. His countenance, too, fails to indicate the oppression that is constantly exhibited in fever, and which a practised eye can so readily detect."

The exemption of the nervous system, however, is not always such as is here described; the tubercle not unfrequently extends to the membranes of the brain; and, whether this be the case or not, that organ is apt to be oppressed and sluggish, as a consequence of the dyspnoea. The following passage on the characters of cough is also sufficiently good and characteristic:—

"It is not perhaps easy to describe these different varieties of cough in so many words; the only true method of becoming familiar with them, is to use your own ears on every occasion, and not to be satisfied with the simple statement, that the patient's cough is troublesome, or the reverse. In pleuritis the cough is short, impeded, and suppressed; in bronchitis, full and open; in hoop-

ing cough, it occurs in paroxysms threatening instant suffocation; in consumption, it varies with the stage; in the early period, before softening has taken place, it is short and dry; in the advanced period it is hollow and moist; in pneumonia, on the other hand, it is mixed; the hardness and restriction which arise from pulmonary consolidation being modified by the relief that follows the effort of expectoration. In the case of acute phthisis, the cough has all the hardness, frequency, and absence of expectoration that characterise the early period of tubercular irritation, while the minuteness of the deposit, as contrasted with its ordinary development, causes it still to present some resemblance to the cough of bronchitis."

The remaining lectures, four in number, are upon different chest affections; and although characterized by all the author's good qualities, and some of his faults, contain nothing on which we are desirous of remarking, excepting the assertion in page 70, that pneumonia may be distinguished from tubercle by the enlargement which takes place in the lung. We thought that Laennec had long ago demonstrated that no such enlargement takes place; and in the case here related, we have no doubt that it was owing to the pleurisy.

The reader will not fail to have observed that the present volume is scarcely a safe one to put into the hands of a young student, although, with a little time, discrimination, and weighing of particular expressions, it might readily be purged of its errors. We hope that Dr Duncan's next production will demand a more unqualified approval.

Surgical Anatomy. By JOSEPH MACLISE, Surgeon. Fasciculus I.
London: 1848. Folio.

THIS simple and short title denotes the beginning of a very extensive and important work on regional anatomy by Mr MacLise, who is well known to the profession through his beautiful illustrations of Mr Quain's work on the arteries, and through his own works on comparative anatomy. Mr MacLise is indeed a very thorough anatomist, and has studied the subject in the most scientific and enlarged spirit, in relation both to the theoretical and practical departments of science; and the present work, as a system of relative anatomy, is not merely intended for the surgeon, but as an illustration of whatever, either in surgery or medicine, depends upon an intimate knowledge of the relations of the different parts of the human frame. In a singularly nervous preface, he insists upon the importance and beauty of anatomy as a science, when cultivated in its fullest and widest acceptance, and seems to chafe at the very idea of the narrow limits of mere human anatomy which the present work imposed upon him, and at the anatomical terms of the middle ages, which he characterizes as a "barbarous jargon of words, barren of all truthful signification."

"If there may be any novelty now-a-days possible to be recognized upon the
NEW SERIES.—NO. XXX. DEC. 1848. 3 G

out-trodden track of relative anatomy, it can only be in truthful and well-planned illustration. Under this view alone may the anatomist plead an excuse for reiterating a theme which the beautiful works of Cowper, Haller, Hunter, Scarpa, Soemmering, and others, have dealt out so respectably. Except the human anatomist turns now to what he terms the practical ends of his study, and marshals his little knowledge to bear upon those ends, one may proclaim anthropotomy to have worn itself out. Dissection can do no more, except to repeat Cruveilhier."

With all his scientific fervour, however, Mr Maclise seems fully to appreciate the practical nature of his subject; and his love of the new spirit has not induced him to sacrifice the old terms, which, we suspect, are in fact as well adapted to our wants as any thing that could be substituted for them.

The plates in the present fasciculus are four in number; one being devoted to the thorax, and three to the neck and its surgical regions. They are all extremely well executed; and Plates II. and III., in particular, are in many respects among the best planned dissections of the neck that we have seen. The descriptions fully correspond in clearness and usefulness with the plates; only we have at times to complain of certain eccentricities and imaginative peculiarities of style, which Mr Maclise ought not to throw away upon a work of *mere human* anatomy. By a little care, the succeeding parts may be easily kept free of every thing of this kind. We shall look for the results of the *perfervidum ingenium* of the author rather in well-planned and delineated dissections, than in such traits of manner; and we shall expect to observe in the succeeding numbers many excellent new views of hackneyed subjects. Of this Mr Maclise has given fair promise.

One most important merit of the work is its cheapness; the four large coloured plates of this fasciculus, with the descriptive letter-press, only costing five shillings. The paper and execution are beautiful, and the work is in every way creditable to the enterprise of Mr Churchill, the publisher.

A Hand-Book of Physiology. By WILLIAM SENHOUSE KIRKES, M.D., assisted by JAMES PAGET, Lecturer on General Anatomy and Physiology at St Bartholomew's Hospital. London, 1848. 12mo, pp. 705.

THIS work was originally planned as a hand-book, on the basis of Müller's Physiology; and was intended to render the facts and views in that work more available for the purposes of students, by throwing them more into a form adapted to the portion of time usually devoted to physiology in an academical course. In proceeding, however, the authors found it necessary to adopt a bolder principle, giving the work a form and arrangement of its own, and drawing the materials from the original sources of supply, in the departments

which chiefly require it. In this they have done well. The form of Müller's Physiology has been always felt to be complex and ill arranged, especially for beginners; although, as a repository of physiological information, it is decidedly the best yet published.

From an examination of several of the articles in this volume, we are enabled to say, that the authors have succeeded in producing a work well adapted for students. The copious references made, both to elementary works, and to original papers, are most valuable; more especially as this species of research is of great importance to students, and is apt to be neglected by the authors of manuals. The volume is also well illustrated, many of the wood-cuts being those of Müller's Physiology.

Surgical Experience of Chloroform. By JAMES MILLER, F.R.S.E.,
Professor of Surgery in the University of Edinburgh. 8vo, pp.
60. 1848.

MR Miller's pamphlet has a more than ordinary claim on the attention of the profession, from the circumstance that he was the first to test the value of the new anæsthetic in operative surgery. It was evident to all who witnessed the operation in question, that a new era in the history of anæsthesia had begun, and, before many days had elapsed, chloroform was in general use over Europe and America.

Mr Miller has had occasion to employ chloroform extensively, both in public and private practice, and he has embodied the result of his experience in this pamphlet, which contains by far the best account yet published of its surgical applications. The first nine sections are devoted to details in reference to the purity of the chloroform, its mode of administration, &c. The remainder of the pamphlet is subdivided into two parts; the first of which treats of the advantages derived from anæsthesia in the performance of operations, and the second enumerates those affections, in which, though unconnected with operations, important benefits are obtained from the use of chloroform. The reader will derive much useful information from the perusal of the entire pamphlet; but we recommend especially to his notice the sections on the use of anæsthesia in dislocations, in the examination of injuries, in the treatment of irritable stricture, in the operation of sounding, and in stone of the bladder complicated with diseased kidney, under which heads Mr Miller's observations are of a highly practical and excellent character.

We had contemplated a more lengthened notice of this valuable essay, and had marked several passages for insertion; but the subject of chloroform has already occupied a large space in our pages.

Part Third.

MEDICAL NEWS.

THE CHOLERA.

THE epidemic which we announced in our last month's publication as having invaded this country, has continued to rage with great virulence in Edinburgh, where the number of cases now amounts to between seven and eight hundred (including Leith), of which nearly a half have already proved fatal, about 230 remaining under treatment. A severe epidemic broke out at Loanhead (a village six miles to the south of Edinburgh), and many cases have also occurred at Lasswade, Gilmerton, Portobello, Cockpen, and other villages of the neighbourhood. Several cases have been reported in Glasgow, mostly in very crowded and miserable localities: and the disease is also reported to exist in Dumfries as well as in Falkirk, a town whose sanitary condition is far from satisfactory. In England the number of cases has not approached that of the northern division of the kingdom; and, although in London the number has been considerable, yet, relatively to the population, it is very small compared with Edinburgh. On the week ending October 4th, 65 deaths were recorded in the Registrar-General's returns from cholera in London; on November 11th, 62; on November 18th, 54. The epidemic appears therefore, on the whole, to be decreasing in violence in London. Every where the disease appears to have been almost confined to the most wretched and miserable of the population; a few trivial exceptions only to this rule having occurred in London and Edinburgh. The mortality has been every where nearly the same,—viz., about two-thirds of the recorded cases which have been brought to a termination. It is probable however, that no trustworthy returns of the whole cases in any place have been yet published.

On the continent it has declined in a great measure at St Petersburg, Berlin, and Hamburg, where, however, cases still occur. On the other hand, Rotterdam has been attacked with great severity since the 1st of October, and Dantzic, from a similar date, has suffered under a still more severe visitation. It is stated that in the small town of Gartz, in the district of Stettin, there has been the extraordinary mortality of 102 persons out of a population of 700.

A considerable number of cases of a choleroïd disease prevailed in the beginning of the month in Dunkirk; and a few have occurred in the neighbouring villages, and in Calais. M. Magendie was sent by the Academy of Sciences to investigate this alleged ingress of the disease into France; and reported the cases not to be true epidemic cholera. If we may trust the report of a discussion in the *Société de Médecine Pratique*, several cases having the strongest resemblance to the disease appear to have occurred in Paris; but there seems to be, on the part of the French physicians, a strong disposition to doubt or deny its presence.

The General Board of Health has displayed considerable activity, but not all in the right direction. The truth is, that it has been hard beset. Composed, as it is in great part, of non-medical members, and forced to act in an emergency, the single medical head connected with it, has not been able to save it from being thrown upon the suggestions and recommendations of busy-bodies; the effect of which was the framing of a document full of inconsistency, and founded on most imperfect knowledge of the real views of the profession. The College of Physicians of Edinburgh was the first to disclaim the views of the Board of Health, as we noticed in our last Number; and this was followed by a most wise and temperate document on the part of the London College of Physicians, recommending hospitals and houses of refuge, and showing the absurdity of the attempt to dictate particular dietetic systems to persons who may be very dependent on many of the forbidden articles. Nearly the whole medical press of the country having likewise joined in tolerable unanimity on this subject, the General Board has been induced to remodel its in-

structions ; and we have now before us a set of additional directions for England, and another for Scotland, in which particular orders are given for the construction of cholera hospitals and houses of refuge, and for the employment, where necessary, of additional medical assistance.

We are happy to find that all the medical authorities have agreed to consider the question of contagion an open one, and have concurred expressing sentiments of a moderate character on this subject, closely corresponding with those which we endeavoured to impress upon our readers in the last Number of the *Journal*. We hope that in all hospitals and public institutions, facts relating to the propagation of cholera will be scrupulously and carefully recorded. Reports should be drawn up of the *whole* of the nurses and other persons exposed to contagion in such institutions ; with the view of ascertaining the proportion in which they are affected with the disease. We believe that a large body of returns of this kind would be of more essential service than any other species of evidence on the subject of contagion.

A committee of the Edinburgh College of Physicians and Surgeons has been appointed, to procure evidence in relation to the present epidemic of cholera in Edinburgh and Scotland. Circulars have been issued to all the medical practitioners in Edinburgh, and the adjoining villages which have been invaded with cholera, enclosing blank schedules, to be filled up with information in a short form, in regard to any cases which may occur. We take this opportunity of impressing upon medical men the importance of attending to this point, as the labours of the committee can only be rendered available for scientific purposes, by the co-operation of a large number of persons ; and the trouble imposed on them by filling up the schedules, is, from the simplicity with which these are drawn up, extremely trifling.

The most remarkable circumstance as to the treatment of cholera, arising out of the present epidemic, is the employment of the inhalation of chloroform in ten cases successively in Peckham Asylum, without a single fatal result. This favourable experience has not been borne out in Edinburgh, where this remedy has been very assiduously tried in the Cholera Hospital without the least apparent good effect. The weekly and daily journals are full of remedies, but few of these rest on any large experience.

We have received a number of publications on the treatment of cholera, some of which we mean to notice next month. In the mean time, we shall not add one word to the remarks of Dr Robertson, in a paper which will be found among our original communications, and which is the result of by far the largest experience yet furnished by the present epidemic. We have great pleasure in bearing witness, from personal observation, to the care and judgment with which Dr Robertson's experimental trials have been conducted.

We have given, in the Practice of Physic section of the *Retrospect*, the results of practice in India for some years past in cholera—as collected from a large series of medical reports by Mr Samuel Rogers. We propose to notice this important work more fully in next Number.

GUANO A "DRUG" IN THE MARKET.

DR VINCENZO GONZALES, writing to the Academy of Bologna from New Grenada, states that guano, either in the form of baths, frictions, or decoction, is in that country successfully used against lepra, (probably elephantiasis arabicum.) The patient is first given an ounce of guano in a quart of barley-water, taking care to mix well. The next day he has a bath at the ordinary temperature, to which are added ten ounces of guano in fine powder. The patient then is put into a warm bed, and an hour afterwards frictions with guano ointment are made. On the day when the bath and the frictions are used, no guano is taken internally, and *vice versa*. The patients sleep on mattresses stuffed with guano. The government of New Grenada has sent a circular all through the country, to recommend guano against lepra. *Is this a Transatlantic hoax ?*

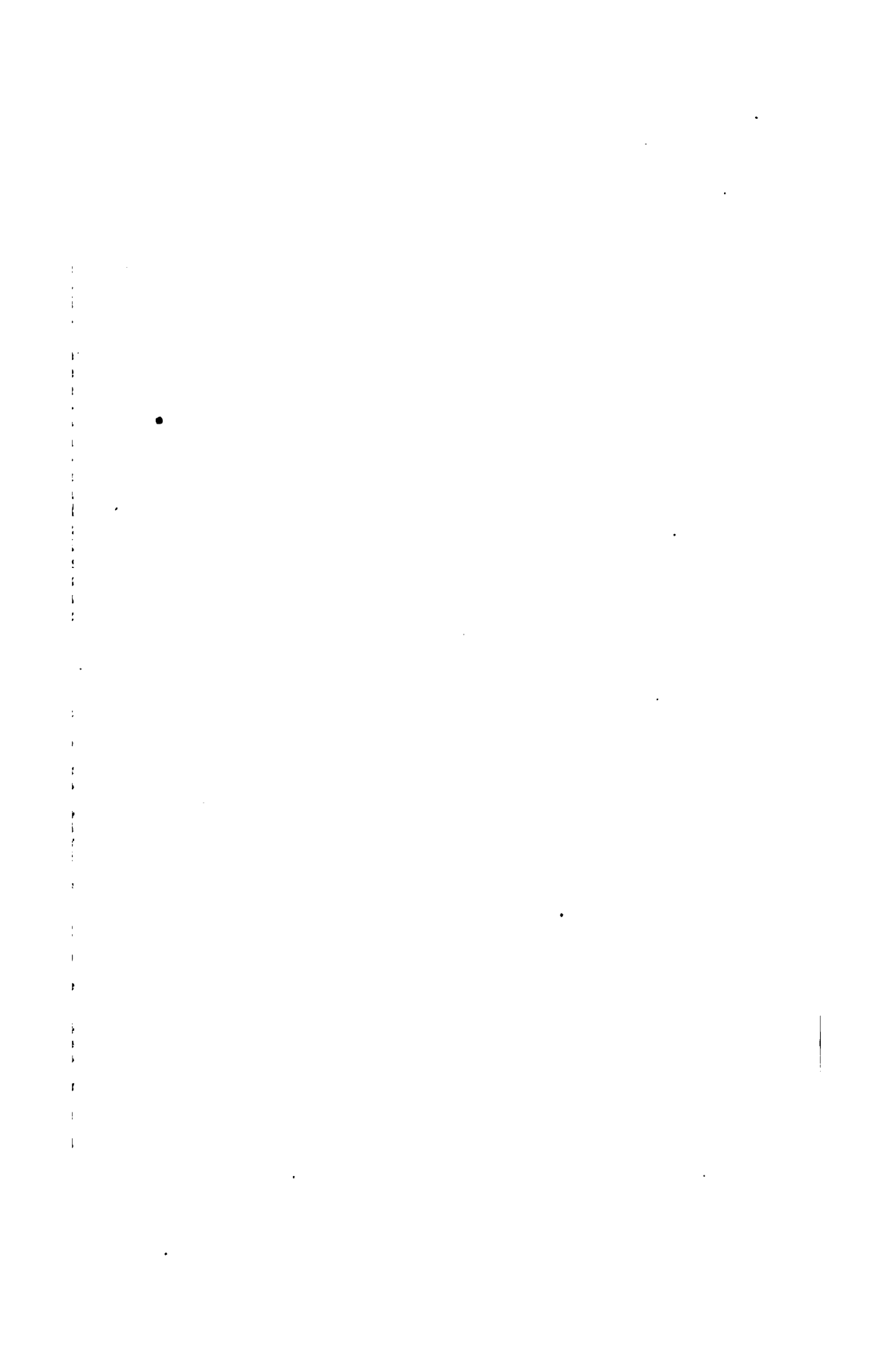
MEDICAL JOURNALISM IN ITALY.

OUR readers will be surprised at the following numerous list of medical journals and transactions of medical societies published in Italy. Three appear in the Papal states, *Il Bollettino Medico of Bologna*, *Gli Annali Medico-Chirurgici of Rome*, *Il Raccoglitore Medico of Fano*; three in the Neapolitan states, *L'Osservatore Medico*, *Il Filiale Sebazio*, and *Il Sarcone*; two in Lombardy, *Gli Annali Universali di Medicina*, and *Gazzetta Medica of Milan*; three in Tuscany; one in Piedmont, *Journal de la Société Medico-Chirurgicale de Turin*; lastly, two in the Venetian states, *Il Giornale per Servire ai Progressi*, and the *Memoriale della Medicina Contemporanea*; in all fourteen, and we believe the names of two or three are omitted. This formidable *journalistik* is one among other injurious effects of the subdivision of Italy into so many independent principalities, each of which must have its medical school and journal. The consequence of this ruinous competition is a great dearth of good original matter, and, generally speaking, a low standard of excellence in the majority of the publications above named, which consist chiefly of extracts from foreign, and more especially the English journals. In the pages of two or three, however, which form honourable exceptions to this remark, may be found valuable contributions to medical science. For the information of the reader, we may state, that probably the best, as it is the oldest, Italian medical journal, is the *Annali Universali di Medicina*, published at Milan. Few of the others find their way to England; but excellent analyses of their more important papers are furnished by the *Gazette Médicale* and *L'Union Médicale* of Paris, from which they are again occasionally transplanted, for the benefit of our readers, into the "Monthly Retrospect."

BOOKS RECEIVED.

1. Graves' Practice of Medicine. Edited by J. Moore Neligan, M.D. Dublin. 8vo. 1848.
2. Hassall's Microscopic Anatomy. Part XIV. 8vo. London. 1848.
3. West on the Diseases of Infancy and Childhood. 8vo. London. 1848.
4. Neligan on Diseases of the Scalp. 12mo. Dublin. 1848.
5. Handbuch der Gewebelehre. By Dr Jos. Gerlach. 8vo. Mayence. 1848.
6. School Chemistry. By Dr Thomson. 12mo. London. 1848.
7. Guy's Hospital Reports, Oct. 1848. 8vo.
8. Peacock on the Influenza of 1847-48. 8vo. London. 1848.
9. Lee's Clinical Midwifery. 12mo. Lond. 1848.
10. Chapman on Ulcers of the Leg. 12mo. London. 1848.
11. Taylor's Medical Jurisprudence. Third edition. 12mo. London. 1848.
12. Bowman's Introduction to Practical Chemistry. 12mo. London. 1848.
13. Miller on Surgical Experience of Chloroform. 8vo. Edin. 1848.
14. Bainbrigg on Chloroform. 8vo.
15. Pharmacopœia ad usum Nosocomii Phthisicorum et Pectoris Morbis Aegrotantium accommodata. 12mo. London. 1848.
16. Hygiea, Medicinsk och Pharmaceutisk Månads-Skrift, Tionde Bandet. 8vo. Stockholm. 1848.
17. Tylee, Practical Observations on Galvanism, &c. 12mo. Bath. 1848.
18. Bartlett, Inquiry into the degree of certainty in Medicine. 8vo. Philadelphia. 1848.
19. Green—Observations on the Pathology of Croup. 12mo. New York. 1848.
20. Medical Report of the Lancaster Lunatic Asylum. 8vo. Lancaster. 1848.
21. Peacock on Influenza. 1848.
22. Pathological Society's Reports, 1847-8. 8vo. London.
23. Ethnological Journal. No. 6.
24. Kirkes' Handbook of Physiology. 12mo. London. 1848.
26. Carpenter on Medical Study. 8vo. London. 1848.
27. Keir, Leared, Billing, Paterson, Charles Bell, and Grieve, on Cholera.
28. Journal of Public Health. 1847-8.
29. Transactions of the American Medical Association. 8vo. Philadelphia. 1848.
30. MacLise—Surgical Anatomy. Folio. London. 1848.
31. Tracts for the Improvement of our Popular Literature. No. 3.
32. The Idea of Life. By S. T. Coleridge. 12mo. Lond. 1848.
33. Gay on Femoral Rupture. 4to. Lond. 1848.

Report of Edinburgh Medico-Chirurgical Society's meeting next month.



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